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Book Reviews

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In This Issue

By interesting coincidence, the author of the essay on random coefficients in *The New Palgrave: A Dictionary of Economics* (see review) is also senior author of the first article in this issue. Swamy, Conway, and LeBlanc present the first of a series of three articles on the arguments for, and uses of, stochastic coefficients in economic modeling. Why stochastic coefficients? Because economic relationships are not as fixed and deterministic as traditional models would imply. Forecasters and policymakers run the risk of serious mistakes by assuming fixed coefficients.

Serial articles are not typical for the Journal, although the precedent was set in the very first volume. We have chosen to serialize the stochastic coefficients article not only to accommodate its length but because there is a natural break between the arguments against fixed coefficients, the description of and arguments for stochastic coefficients, and potential in applications. We believe the articles in the spring, summer, and fall issues will make an important contribution to the perspectives of our econometric work in agriculture.

In comparing Crop Reporting Districts (CRD's) and Major Land Resource Areas as aggregate regions in production analysis, Offutt used a random coefficient model to test for aggregation bias. For Illinois, at least, her research supports the use of CRD's as a unit of aggregation for crop production data. Hahn addressed the problem of estimating aggregate demand functions taking into account different preferences associated with the distribution of income. He had only limited information with which to relate income distribution and demand for meats. His *ad hoc* moment-generating function enabled him to estimate successfully the effects of income distribution on the demand for beef, pork, and chicken. He compares his method with alternatives.

Book reviews in this issue include a critique by Reilly of *Macroeconomic Impacts of Energy Shocks*. In his generally favorable review, Reilly asks the important question, applicable to many areas of analysis: ". . . do we believe the models so that the experiment tells us how the economy reacts or do we use the experiment to help us produce better models?" Despite some ambiguity in the answer to his question, he recommends the book for its contribution to modeling macroeconomic shocks.

Small banks are extremely important to agriculture and rural communities so the 1982 deregulation had significant impacts in the Midwest and South, according to Hiemstra's review of *The Future of Small Banks in a Deregulated Environment*. The scholarly book contains a useful review of empirical data and literature.

Ribaudo reviews *Agriculture and the Environment*, a book from Resources for the Future appearing as a policy annual. While acknowledging major contributions of several papers in the compilation, he notes some important omissions pertaining to the Conservation Reserve Program, low-input agriculture, and biotechnology.

Mayer praises *Agricultural Marketing Enterprises for the Developing World* for its summary of "a set of useful steps that improve the marketing process" and for its 26 case studies of marketing situations illustrating a wide variety of conditions. He supports the author's position of shifting responsibilities for marketing from Government to individuals.

Words economists live by are arranged alphabetically, with essays, in *The New Palgrave: A Dictionary of Economics*. I have sampled its 2,000 essays (I did not count them, but instead took the word of the editors). It is a great reference book. Have a look at it.

Gene Wunderlich

The Stochastic Coefficients Approach to Econometric Modeling Part I: A Critique of Fixed Coefficients Models

P.A.V.B. Swamy, Roger K. Conway, and Michael R. LeBlanc

Abstract. *Stochastic coefficients models can provide accurate agricultural sector forecasts and useful policy analysis. Coefficient variation may occur for many reasons including aggregating over micro units, omitting variables, using an incorrect functional form, and allowing for a dynamic economic theory of optimizing behavior. In the first article in this series of three, we address the logical problems with fixed coefficients models. A number of auxiliary and possibly contradictory assumptions are imposed on econometric models to make them empirically manageable. In the second and third articles, we will show how stochastic coefficients models eliminate the logical problems associated with fixed coefficients models.*

Keywords: *Stochastic coefficients, fixed coefficients, aggregation, classical logic, probabilistic logic, evidential interpretation.*

> "The coefficients arrived at are apparently assumed to be constant for 10 years or for a larger period. Yet, surely we know that they are not constant. There is no reason at all why they should not be different every year."
>
> John Maynard Keynes, 1938[1]

The economics profession recognizes increasingly that the classical regression assumption of constant slopes is dubious. Indeed, if there were not this recognition, why adjust the constant terms or add factors to improve forecasting and policy simulations? By tampering with their models, econometricians implicitly acknowledge more variability in their models than they can capture by classical autoregressive errors. Furthermore, the necessity to apply such first aid to classically estimated models with or without deterministic shifts in coefficients appears to have increased over the past decade.

Many econometricians today remember the sixties as halcyon years when policymakers believed they could "fine-tune" the economy and determine an optimal policy mix, first, by simulating their models of conventional type under a variety of policy assumptions and, then, by reviewing the influence of their policy assumptions on the pertinent endogenous variables. The experience of the seventies irrevocably altered the sanguine attitude of those policymakers. Severe and dramatic structural shocks and shifts such as crop failures, the Soviet grain deal, the Soviet grain embargo, the poor Peruvian anchovy harvest in 1973, the movement from fixed to floating exchange rates, wage and price controls, the two energy price shocks, the payment-in-kind (PIK) program, and institutional changes in the financial sector wreaked havoc on the vain attempts by econometricians to provide reliable and consistent policy recommendations from the results of their models with fixed or deterministically changing slopes. Stochastic coefficients models have been developed to address these and other problems.

This article is the first of three articles devoted to the evaluation of stochastic coefficients models. Interest in stochastic coefficients modeling is no longer confined to the general economic literature. In the past few years a number of agricultural economists have discussed and applied these new methods in agricultural settings.[2] We believe many benefits for agricultural decisionmakers and the agricultural community will flow from these efforts. Our series will highlight the variety of models available, their usefulness, their assumptions, their limitations, and their possible contradictions. We first demonstrate that stochastic coefficients modeling rests on firmer philosophical and logical foundations as an econometric methodology than does conventional constant coefficients modeling. We list the reasons why coefficients may be stochastic and discuss the inherent

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[1] Keynes' comment on a proof copy of Jan Tinbergen's *Business Cycles in the United States of America*, as quoted in (*23*, p. 286). Italicized numbers in parentheses refer to items in the References at the end of this article.

[2] See (5, *7*, *8*, *9*, *10*, *25*, *32*, *33*).

weaknesses in the conventional econometric approach to modeling that lead to logical inconsistencies. In the second article, we will show how a methodology based on stochastic coefficients in a time-varying mode avoids this logical quagmire. However, the generality of stochastic coefficients models leads to certain practical problems which we will also discuss. These problems become less serious if we recognize that the real aim of statistical inference is usually to generate a prediction about the value of some future observables, as shown in the second article.

Why Should Coefficient Variation Occur?

There are a number of theoretical and empirical justifications for specifying a stochastic coefficients model. First, there is no *a priori* reason why the "true" coefficients themselves cannot be generated by a nonstationary or time-varying random process, as the opening quotation from Keynes shows. Second, omitted variables that exhibit nonstationary behavior and that are not orthogonal to the included variables can induce variability in the coefficients of included variables (*11*). Third, it is a conventional econometric practice to use proxy variables in place of unobservable explanatory variables. As we know, proxy variables will only imperfectly capture changes in the economic behavior of the true variable, and the relationship between the "true" variable and its proxy may change in time. Fourth, aggregation over micro units can induce variation. It is highly restrictive to assume the aggregation weights of microeconomic units will not change over time (see *36*, *37* for a discussion of this point). There are surely few observed events that are not already the outcome of some aggregation. Therefore, since the topic is important, let us consider the aggregation issue in detail.

A problem with constant coefficients arises naturally from aggregation in economics. Neoclassical economists begin with the assumption that economic agents optimize. From there, microeconomic models of an individual consumer demand (or factor input demand schedules for a firm) can be derived from a consumer utility function (or firm output or profit function) (see *31*).

Because micro data are typically not available, researchers are forced to aggregate up to a macroeconomic model. Here one assumes a homogeneous structure when constant coefficients are employed. However, it is not reasonable to assume the same utility function for all individuals or that every firm in a given industry faces the same objective function. Instead, it is more reasonable to assume coefficients will change across individuals and firms. Furthermore, it is more reasonable to assume coefficients will vary over time as a result of changes in taste and technology, among other reasons.

To explore the aggregation issue, we first state a general micro equation as:

$$y_{it} = \sum_{j=1}^{k} \beta_{ijt} x_{ijt} \quad i = 1, 2, \ldots, n_t \; ; \; t = 1, 2, \ldots, T \tag{1}$$

where y_{it} and the x_{ijt} represent micro units' dependent and independent variables, n_t is the number of micro units, i indexes a cross-section unit, and t indexes time. To allow for a combined intercept and additive error term, we set $x_{i1t} \equiv 1$; that is, β_{i1t} represents the sum of the intercept and an error term. Since micro data are frequently not available, we aggregate equation 1 over i to obtain a macro function. This aggregation procedure requires some extra assumptions.

One way to aggregate the data described by Swamy, Barth, and Tinsley (*28*) is as follows. Subtracting and adding the function $\sum_{j=1}^{k} \beta_{jt} x_{ijt}$ with time-dependent coefficients on the right side of equation 1 gives an equation that, after summing over i and dividing through by n_t, shows the aggregate function to be:

$$\frac{1}{n_t} \sum_{i=1}^{n_t} y_{it} = \sum_{j=1}^{k} \beta_{jt} \frac{1}{n_t} \sum_{i=1}^{n_t} x_{ijt}$$

$$+ \frac{1}{n_t} \sum_{i=1}^{n_t} \sum_{j=1}^{k} (\beta_{ijt} - \beta_{jt}) x_{ijt} \tag{2}$$

This equation can be written as:

$$y_t = \sum_{j=1}^{k} \beta_{jt} x_{jt} + \xi_t \tag{3}$$

where:

$$y_t = (1/n_t) \sum_{i=1}^{n_t} y_{it},$$

$$x_{jt} = (1/n_t) \sum_{i=1}^{n_t} x_{ijt}, \text{ and}$$

$$\xi_t = \frac{1}{n_t} \sum_{i=1}^{n_t} \sum_{j=1}^{k} (\beta_{ijt} - \beta_{jt}) x_{ijt} \tag{4}$$

Equation 3 with ξ_t suppressed is theoretically plausible if ξ_t converges in probability to zero for every t. This convergence holds if and only if both equation 1 and the condition that $E[|\xi_t|^r/(1 + |\xi_t|^r)] \rightarrow 0$ as $n_t \rightarrow \infty$ for some $r > 0$ and every t hold (see *15*, p. 69). An alternative set of conditions for the convergence of ξ_t to 0 in probability appears in (*24*). Serfling (*26*) states five theorems that also give alternative sets of conditions for the stochastic convergence of ξ_t to zero.

In the case where all the elements of the individual-invariant coefficients β_{jt} ($j = 2,...,k$) corresponding to the slopes of equation 1 are also time-invariant, equation 3 reduces to:

$$y_t = \beta_{lt} + \sum_{j=2}^{k} \beta_j x_{jt} + \xi_t^* \qquad (5)$$

where:

$$\xi_t^* = \frac{1}{n_t} \sum_{i=1}^{n_t} (\beta_{ilt} - \beta_{lt}) + \frac{1}{n_t} \sum_{i=1}^{n_t} \sum_{j=2}^{k} (\beta_{ijt} - \beta_j) x_{ijt}$$

If this ξ_t^* converges in probability to zero, then equation 5 reduces to:

$$y_t = \beta_{lt} + \sum_{j=2}^{k} \beta_j x_{jt} \qquad (6)$$

Equation 6 is the same as an aggregate equation of the conventional type because, in the traditional fixed-coefficients models, the intercept may be interpreted as a random coefficient. We may consider the quantity $\beta_{lt} = \beta_l + u_t$ as a random variable with mean β_l, whereas the slope coefficients $\beta_j (j = 2,...,k)$ are fixed.

As is well known in statistics, we cannot achieve stochastic convergence without centering and scaling the variables. There are different ways of centering and scaling (see *15*, pp. 68, 280). The definitions of ξ_t and ξ_t^* are different because in these definitions the micro coefficients are centered differently. This difference is the only reason why conditions for the stochastic convergence of ξ_t^* to zero are more stringent than those for the stochastic convergence of ξ_t to zero. The point here is that the term ξ_t is more likely to converge in probability to zero and will do so more quickly, if the micro coefficients are centered by subtracting time-varying coefficients as in equation 4 than if they are centered by subtracting fixed coefficients as in equation 5. This advantage by centering with time-varying coefficients means that the aggregate equation 3 with ξ_t suppressed and with variable slopes can exist under weaker conditions than the aggregate equation 6 with fixed slopes and random intercept. For any practical work, the existence conditions are important because a model that does not exist could not have generated our data and should not be used for empirical analysis. Thus, if one wants to assume weaker conditions to show the existence of an aggregate model and to avoid aggregation bias, one should use aggregate models with time-varying slopes.

A fifth justification for specifying stochastic coefficients is that coefficient variation may occur as a result of an incorrect functional form. Because the true functional forms are usually unknown, equation 1 is preferable to a linear equation with fixed slopes because the former can trace any nonlinear path depending on the time profiles of β_{ijt}'s. As Rausser, Mundlak, and Johnson observe, "The approximation of highly nonlinear 'true' relationships by simpler functional forms, along with observations outside the narrow sample range, provides perhaps the strongest motivations for a varying parameter structure" (*25*). Goldberger (*14*, pp. 108–11) also makes the same point while commenting on the Rotterdam school demand models. Finally, conventional econometric models may be in contradiction to the dynamic economic theory of optimizing behavior. A change in economic or policy variables will result in a new environment that will, in turn, lead to new optimal decisions and a new micro and macroeconomic structure. This formalization of Keynes' intuitive insight is the contribution of Lucas (*20*). As Lucas and Sargent note, this "[e]quilibrium theorizing . . . readily leads to a model of how process nonstationarity and Bayesian learning applied by agents to exogenous variables lead to time-dependent coefficients in an economic agent's decision rule" (*21*). This dynamic optimizing behavior further justifies our use of micro equations of the form of equation 1 with all its coefficients varying across individuals both at a point in time and through time. As we have shown, using an aggregate equation with all its coefficients varying over time represents an attempt to deduce an aggregate equation from these plausible micro equations under weaker conditions. Equation 3 with ξ_t suppressed can be called a stochastic coefficients model if its slopes are stochastic. We will see that another advantage of the variable coefficients models over the fixed-coefficients models is that the former are free from the contradictions that may exist in the fixed-coefficients approach.

The Logical Cracks in Conventional Econometric Models[3]

Although fixed coefficients models appear simple, there are serious logical problems with this mode of modeling

[3] This section is based on (*30*).

that further support using variable coefficients models. To illustrate, let us review the traditional models and their assumptions.

Traditional econometric models posit two sets of assumptions. First, there is a set of behavioral assumptions relating to the economic agents under study. Second, there is a set of additional assumptions determined by the form of simplifications imposed by empirical necessity on these behavioral assumptions.

The familiar matrix formulation of a linear system of behavioral equations is:

$$Y\Gamma + XB = U \tag{7}$$

where Y is the $T \times G$ matrix of observations on G endogenous variables, Γ is the $G \times G$ matrix of constant coefficients of the endogenous variables, X is the $T \times K$ matrix of observations on the K exogenous variables, B is the $K \times G$ matrix of constant coefficients of the exogenous variables, and U is the $T \times G$ matrix of disturbances. Assume also that U is mean independent of X, that is, $E(U|X) = E(U) = 0$ and the $E(U|Y) \neq E(U)$ in general. The assumption of mean independence is stronger than the assumption of uncorrelatedness and weaker than the assumption of independence (see *6*).

If Γ is nonsingular, we can obtain the reduced form as:

$$Y = X\Pi + V \tag{8}$$

where $\Pi = -B\Gamma^{-1}$ and $V = U\Gamma^{-1}$.

How should one evaluate such a model or econometric models in general? Boland suggests "the only *objective* and nonarbitrary test to be applied to theories or models is that of logical consistency and validity" (*4*, p. 24). According to Boland, although logically valid models can be true, logically invalid models cannot be true. (Further discussion of this topic appears in *29*.) We, therefore, document the eight explicit or implicit assumptions an econometrician must make to estimate models like equation 7 including some assumptions that are not well known.

First, the conditions of Kagan, Linnik, and Rao's (KLR's) lemma given in (*6*) hold for a given set of endogenous and exogenous variables.

This lemma provides conditions for the existence of a set of linear regression equations, or linear population regression functions of the form of equation 8 between each of a set of endogenous variables and a set of exogenous variables in every time period t. This result suggests that a dependent variable in time t has an expectation linear in, and a variance independent of, the conditioning vector of independent variables. We have already pointed out the importance of existence conditions for empirical work. When a condition of KLR's lemma holds, described in (*6*, p. 6), the disturbances in equation 8 are mean independent of the independent variables X and our assumption about X in equation 7 is correct.

Second, the rank of X is K if $K \leq T$ and the rows of Π corresponding to the linearly dependent columns of X are null if $T < K$. Alternatively, the matrix Π satisfies some exclusion restrictions even when the rank of X is K.

Third, for $j = 1, 2, \ldots, G$, the $G - G_j - 1$ elements of the jth column of Γ and $K - K_j$ elements of the jth column of B are zero. One element of each column of Γ is unity, and Γ stays nonsingular even after these restrictions are imposed.

Fourth, the rank of a specific $(K - K_j) \times (G_j + 1)$ submatrix of Π, denoted by $[\pi_j^*, \Pi_j^*]$, is G_j for $j = 1, 2, \ldots, G$.

Fifth, the matrix U is stationary or nonstationary, and the distribution of U is normal or nonnormal.

Sixth, prior distributions of the unknown elements of Γ, B, and the covariance matrix of U are of a particular form.

Seventh, either the system in equation 7 is interdependent or triangular-recursive in the sense of Wold (see *1*).

Eighth, parameters θ and η indexing the conditional and marginal distributions in the equation $F(Y,X|\delta) = F_1(Y|X,\theta)F_2(X|\eta)$ are unrelated in the sense of Basu (*2*, pp. 364-66).[4]

Econometricians typically ignore the marginal distribution of X and base their inferences mainly on the conditional distribution of Y, given X. It is difficult to say whether the eighth assumption is satisfied if the marginal distribution of X is ignored. In any case, the Bayesian definition of parameter unrelatedness is clear. In a frequentist (classical) context, the elements of a vector of unknown constants like θ can be said to be unrelated to the vector of unknown constants like η if it is possible to meaningfully isolate all the relevant information about each element of θ contained in the data. Frequentists' definitions of this concept of parameter unrelatedness could not pass Basu's (*2*) careful scrutiny, although the Bayesian definition did.

[4] With an abuse of notation we are using here the same symbol to denote a random variable and the value taken by the random variable.

Econometricians may prefer model 7 to model 8 because the former, unlike the latter, may represent an economic law, in which case it has a natural causal interpretation. This interpretation follows from Feigl's definition of causality as predictability according to a law or set of laws (see *35*).

By means of classical logic, we may establish sufficient conditions for the truth of model 7. Using the standard truth tables, given, for example, in McCawley (*22*), we may evaluate the truth of the eight assumptions and model 7 if each of the constituents of the statements (assumptions one through eight, model 7) is either true or false (but not both).[5] The set (if assumptions one through eight, then model 7, given assumptions one through eight) *semantically entails* model 7 if, in all states of affairs in which assumptions one through eight are all true, model 7 is also true.

It is unfortunate one cannot guarantee that all eight assumptions are either true or false (but not both). Indeed, a serious flaw with fixed-coefficients models depicted in models 7 and 8 is their lack of logical clarity, thereby disallowing any claims about the logical validity of these models. More precisely, the set of behavioral assumptions implied by model 7 and the eight auxiliary assumptions required for estimation may conflict with each other. For example, Swamy (*27*) has shown the restrictions of assumption two imposed on reduced-form coefficients may contradict the identifying restrictions of assumption three imposed on structural coefficients. Furthermore, consider the case when an econometrician does not use all the exogenous variables in the system to compute an instrumental variable estimator. Use of a subset of exogenous variables in a system implies certain zero restrictions on the reduced-form coefficients that might contradict structural identifying restrictions. Therefore, restrictions on the reduced-form coefficients may be implicitly or explicitly imposed even when the matrix of observations on the exogenous variables, X, has full column rank.

Model 7 exists if assumption one is true and $(K - K_j)$ equations, $[\pi_j^*, \Pi_j^*][\gamma_{0j}, -\gamma_{1j}']' = 0$, in $G_j + 1$ unknowns, $[\gamma_{0j}, -\gamma_{1j}']'$, have a unique-up-to-a-fixed-factor-of-proportionality solution for $j = 1,2,\ldots,G$ (see *13*, p. 315).[6] If the equations $\Pi_j^* (\gamma_{ij}/\gamma_{oj}) = \pi_j^*$ are inconsistent, which can happen when $K - K_j \geq G_j$ and assumption one contradicts assumption two or three, then model 7 does not exist under the eight assumptions.

Although, in principle, instrumental variables techniques can be used to estimate model 7 under the eight assumptions, these techniques rest on the hypothesis that certain observed variables used as instruments are truly exogenous and yet have an important influence on the endogenous right-hand variables with nonzero coefficients. These two requirements are contradictory if assumptions one and two contradict assumption three. Instrumental variables estimation in this case would be based on an uneasy compromise where the exogeneity of the instruments is uncertain. Thus, the use of certain observed variables as instruments may imply an unwanted imposition of a set of contradictory model restrictions. The possibility of contradictory model restrictions puts into question the entire class of proofs that have been used to establish the statistical consistency of instrumental variables estimation.

These examples do not end the discussion of possible contradictions among the eight assumptions. The nonstationarity condition of assumption five may imply that some of the errors in model 7 are heteroscedastic. In this case, not only is a restriction of assumption one false but so is assumption eight because those elements of θ that represent the variances of heteroscedastic errors are related to exogenous variables. Thus, assumptions one and eight may contradict the nonstationarity or nonnormality implied by assumption five.

Under the rules of inference adopted by classical logicians, conclusions based on contradictory premises are arbitrary in the sense that both the truth and falsity of a structural model can be inferred from its contradictory premises (see *22*, pp. 29–30).

The above discussion does not capture all legitimate forms of reasoning. There is another system of logic known as probabilistic logic. The set (the eight assumptions) is logically inconsistent if and only if both model 7 and not-model 7 can be inferred from those assumptions. As we have shown, according to classical logic both model 7 and not-model 7 can be inferred from the eight assumptions if these assumptions contradict each other. However, the concept of logical consistency is relative to a given system of rules of inference (or set of general principles) that specifies what conclusions may be inferred from what premises (see *22*, p. 41). Thus, a set of propositions that is inconsistent with respect to the classical system of rules of inference could very well be consistent with respect to the system of statistical rules of inference. Therefore, let us check the consistency of the set (the eight assumptions) with respect to the latter system.

[5] It is clear that, if a statement in an argument is allowed to be both true and false, then the argument involves a contradiction.

[6] Without further elaboration, this statement does not seem to be clear. To illustrate, consider the normalization rule $\gamma_{0j} = 1$ and the case where $K - K_j \geq G_j$. Imposing this rule, we have $\Pi_j^* \gamma_{1j} = \pi_j^*$. The general solution of this equation system is $\gamma_{1j} = \Pi_j^{*-} \pi_j^* + [I - \Pi_j^{*-} \Pi_j^*]z$, where Π_j^{*-} is any generalized inverse of Π_j^* and z is arbitrary. When the fourth assumption holds, the term involving z in the general solution is zero because $\Pi_j^{*-} = (\Pi_j^{*\prime} A \Pi_j^*)^{-1} \Pi_j^{*\prime} A$ is a left inverse of Π_j^* for any nonsingular matrix A. Furthermore, the term $\Pi_j^{*-} \pi_j^*$ is unique since the equation $\Pi_j^* \gamma_{ij} = \pi_j^*$ is consistent.

Suppose the eight assumptions impose restrictions on Γ, B and Π, which make the equation, $F(Y,X|\delta) = F_1(Y|X,\theta)F_2(X|\eta)$, fail; then they are inconsistent relative to probability axioms, since consistency requires that both $F_1(Y|X,\theta)$ and $F_2(X|\eta)$ correspond to some joint distribution of Y and X. Lane and Sudderth (*18*) have constructed conditions for the existence of a joint distribution with given conditional distributions, showing that Bayesian inferences based on a conditional distribution, denoted by $F_3(\theta|Y, X)$, are consistent with some non-Bayesian inferences based on the conditional distribution $F_1(Y|X,\theta)$, if and only if both correspond to some joint distribution, denoted by $F_4(Y, \theta|X)$. These are difficult conditions to check, particularly if a given structural form is nonlinear. In that case the reduced form may not exist and, even if it exists, it may not be unique. Because Lane and Sudderth's consistency conditions may not be satisfied in the nonlinear case, specifying a logically consistent, identifiable nonlinear structural model may be an intractable problem.

Thus, when the linearity assumption in model 7 is relaxed and we assume the structural relationships are nonlinear, an explicit form for the corresponding reduced form may be impossible. One can try, as Gallant and Holly (*12*, p. 699) do, to simply assume the existence of a unique reduced form and add this auxiliary assumption to the ever-expanding laundry list. It is unfortunate that our *a priori* information may be insufficient to conclusively eliminate any possible contradiction between the identifying restrictions on the structure and the conditions for the existence of the nonlinear reduced form or the population nonlinear regression functions of the form $E(y_{it}|x_{1t},..., x_{kt}) = g_i(x_{1t},..., x_{kt})$, $(i = 1,..., G)$. In this example, as with the others, any virtues of the structural equation approach may founder beneath the waves of logical contradiction.

Finally, some of the eight assumptions require modifications if a subset of the right-side variables in model 7 represents unobserved expectations, as in rational expectations models (*34*). In these models, subjective probabilities are equated to the probabilities implied by structural models. Such an equation is unreal because the conditions under which a frequency (or subjective) interpretation of probability is correct are neither necessary nor sufficient for the subjective (or frequency) interpretation of probability to be valid (*28, 29*). Thus, the conjuction of the conditions under which frequency and subjective interpretations are correct is unrealistic and the rational expectations models violate Aristotle's axiom of identity: different statements in an argument should not use different definitions of the same words. Moreover, one can estimate the structural models like model 7 involving the rational expectations variables using the conditional expectations that are generated by stationary time series models. Swamy and von zur Muehlen (*30*) list the conditions under which the stationary time series models of various forms exist. These conditions may contradict the eight assumptions that are required for the existence of linear structural models. As a result, the premises of a conjunction between a stationary time series model and a structural model can be contradictory.

According to classical logic, models with contradictory premises or models that violate Aristotle's axioms of logic are not admissible into logical arguments. There is no guarantee that the premises of a fixed coefficients structural model are not contradictory. By means of probabilistic logic, which is different from classical logic, coherent inferences are not possible if the specifying assumptions underlying a model violate the probability laws. There is also no guarantee that the assumptions underlying a fixed coefficients structural model do not violate the probability laws.

The Lane Question: "When Will We Stop Taking Parameters So Seriously?"

Besides the risk of logical inconsistencies and contradictions, there are deeper philosophical issues to raise regarding the fixed coefficients paradigm. Hypothesis testing traditionally plays a fundamental role in motivating the estimation of fixed coefficients. However, there are serious problems related to the meaning, adequacy, and relevance of the fixed parameter paradigm as typically employed in econometrics.

One may interpret an econometric model by assuming there is a joint probability distribution of the current endogenous variables conditional on the values of the exogenous variables. This distribution can then be written as:

$$P_\theta = F(y_t|x_t,\theta) \tag{9}$$

where θ is a fixed parameter vector taking values in a parameter space Θ and where S is a sample space in which y_t takes on its values.

The appropriateness of inference procedures that can be applied to equation 9 depends on the interpretation of θ. Lane (*17*) defines at least three possible interpretations of the elements of Θ:

1. θ is the distribution equation 9;[7]
2. Θ is an abstract set, and θ simply indexes the distribution equation 9;

[7] Lane meant that only experiments whose sampling distributions are identical share "the same Θ." That is, Lane's interpretation 1 means that the elements of two parameter spaces Θ_1 and Θ_2 are not the same parameter unless the distributions $F(y_t|x_t,\theta_1)$ and $F(y_t|x_t,\theta_2)$ are identical.

3. θ is a possible value for some "real" physical parameter, and the distribution equation 9 is to be regarded as the distribution of the random vector y_t, given x_t, should θ be the true value of that parameter.

The choice Lane poses is important since both classical inferences and decision theory as well as the likelihood principle (LP) require that interpretation 3 holds (see *19*, p. 1, and *17*). The statistical notions of consistency and efficiency are without meaning unless the true value of θ exists. If interpretation 3 does not hold, then the true value of θ may not exist. Furthermore, consistent estimation and testing of θ is impossible unless θ is identifiable because identification is a necessary condition for statistical consistency (*19*, p. 335). However, interpretation 3 also elicits formidable philosophical questions: principally, when—and in what sense—do "real" physical parameters exist? It rather strains credulity to believe there are model-free physical quantities underlying each model parameter without some guidance as to what constitutes "reality" and how "reality" is linked to the mathematics congealed in specific models. It is unfortunate that it is not possible to give such guidance because, for reasons already mentioned, econometricians cannot prove the factual truth of their models (see also *29*). In the absence of such a proof we could be wrong if we interpret the elements of θ as "real" physical quantities.

Interpretation 1 allows no scope for the mixture principle: only experiments whose sampling distributions are identical share the "same Θ." As such, the LP is devoid of interesting consequences under this interpretation (*17*). Interpretation 2, in contrast, allows tremendous scope for mixing. Any two experiments with the same index set can be mixed. In this case, the LP is wrong, as shown by Lane (*17*). We will discuss how stochastic coefficients models relate to interpretation 2 in the second article.

Although econometricians may use economic models to guide the formulation of inference, the inferences have value to us only if they yield useful statements about the real world. Hypotheses stated in terms of the values of θ refer to the real world if θ has a physical interpretation or interpretation 3 is correct. Any evidence against a hypothesis about the world is useful if that evidence has a small probability of occurring when that hypothesis is true. Birnbaum's theory of evidential interpretation is most pertinent here. Evidential interpretations are in the form d_1^* = (reject H_0 in favor of H_1, α_I, β_{II}) and d_2^* = (reject H_1 in favor of H_0, α_I, β_{II}), where α_I is the probability of type I error and β_{II} is the probability of type II error.

This intuitively appealing concept of evidence stating that under no θ shall there be a high probability of outcomes being interpreted as "strong evidence against θ" has been articulated by Birnbaum (*3*) in the following terms:

The Confidence Concept: A concept of statistical evidence is not plausible unless it finds "strong evidence for H_1 against H_0" with small probability α_I when H_0 is true and with much larger probability $(1-\beta_{II})$ when H_1 is true.

Birnbaum's confidence concept makes sense because no satisfactory justification of the choice of a test statistic exists except in terms of the alternatives of interest. Econometricians have all faced situations in which the propriety of considering large or small values of a test statistic as significant rests on the alternatives of interest. There may be little reason to use a given test statistic except in light of certain alternatives.

Birnbaum (*3*) interprets the decision d_1^* = (reject H_0 in favor H_1, 0.01, 0.2) as very strong (but inconclusive) evidence for H_1 against H_0, and he interprets the decision d_1^* = (reject H_0 in favor of H_1, 0.5, 0.5) as worthless evidence. The latter evidence is no more useful than is the result of a toss of a fair coin since the error probabilities (0.5, 0.5) also represent the experiment of tossing a fair coin, with one side labeled "reject H_0" and the other "reject H_1." Consequently, the precise values of α_I and β_{II} go a long way toward interpreting statistical evidence, provided interpretation 3 is correct. Anything less than reporting the values of both α_I *and* β_{II} would not achieve the full disclosure required to make a researcher's study convincing to a critical reader and palatable to a casual reader.

The validity and usefulness of the statistical conclusions based on fixed coefficients models appear to depend on two premises about the nature of inferences in the (S, Θ, P_θ) paradigm. First, the purpose of statistical inference is to make some statement about the "true" value of an unobservable parameter θ on the basis of the observed behavior of certain random variables. Second, θ exists *independently* of the specified model that presumably produced the given data, and information about θ can be separated into two components, one deriving just from the model and "other information" presumably preexisting the model. With all the aggregation and logical inconsistency problems we have pointed out, these two premises are rarely true in the practical situations to which statistical inference is applied, especially in econometrics. In the second article, we discuss the stochastic coefficients approach and show how it addresses these logical problems and philosophical issues.

Conclusions

In this article we have shown that the conventional fixed coefficients modeling approach must employ several and possibly contradictory auxiliary assumptions to be operational. Underlying the traditional technique is a philosophic stance on the nature of parameters that is hard for a social scientist to swallow (and, according to Lane (*16*), a mouthful for the physical scientists as well). In the next article, we will show that stochastic coefficients models ease the number of assumptions and allow a more reasonable foundation to interpret parameters. Thus, we offer a reasonable alternative to the fixed-coefficients approach to counter the pessimistic prospect for econometric modeling suggested in this article.

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Aggregating Crop Production Data: A Random Coefficient Approach

Susan Offutt

Abstract. *Economic analysis of agricultural supply response frequently requires that county crop production data be combined. A random coefficient regression model is used to test for aggregation bias under alternative U.S. Department of Agriculture grouping schemes. The criterion for aggregation is similarity across counties in yield growth rate, taken as a proxy for land quality. Comparison of Crop Reporting Districts (CRD's) and Major Land Resource Areas for Illinois supports the use of CRD's, the form in which supply data are often most readily available.*

Keywords. *Random coefficients, aggregation, supply response.*

The gains from technological advance will not be evenly distributed across agricultural crop producers when the success (as reflected in the size of yield increase) of adoption of a new technology varies (for example, among qualities of land). The nature of this distribution may be important to administrators and policymakers in deciding how to invest in alternative research and development projects for new farm technologies. The evaluation of research activities requires an accurate measurement of investment benefits. Therefore, one should consider potential variations in effects across producers (or across land quality), particularly if systematic variation can be identified with a structural characteristic such as land quality. Based on a preliminary analysis of Illinois corn yield over the past 50 years, gains from technology appeared to vary with land quality (*2*).[1]

To measure the differential effects of technological advance, one may group commodity supply data according to land quality. Soil characteristics, topography, and climate interact to determine the agronomic potential of land. No quantitative index accounts adequately for the contribution of these factors to overall land quality. Thus, over time, yield provides the best available indicator of the underlying resource productivity of field crops (*2*, p. 322). Comparing yields across geographic regions over time enables one to decide how to combine farm- or county-level observations to assess the way technological change affects supply response. Aggregation by State is problematic because it may not be reasonable to assume that political boundaries coincide with variation in the quality of agricultural land. I consider schemes using Crop Reporting Districts (CRD's) and Major Land Resource Areas (MLRA's) as alternative groupings, using a random coefficient regression (RCR) framework to test for aggregation bias.

The issue of the appropriate aggregation scheme for yield data is interesting for two reasons. First, if one is to test hypotheses about the differential effects of technology associated with land quality, the data should be grouped sensibly. Otherwise, hypotheses may be rejected simply because the aggregates do not reflect homogeneous groupings by land quality. Thus, selecting an aggregation scheme is crucial. Second, the general question of data aggregation in the analysis of crop production is important because inappropriate aggregation can obscure salient characteristics of supply response. For example, differences in sensitivity to weather, an important factor in production risk, between two regions may be lost through aggregation. To the extent that such risk varies systematically with land quality, this association affects how much aggregate supply varies and which Government stabilization programs are most helpful on farms.

In a different context, Zellner first noted the possibility of using the RCR framework to test for aggregation bias (*7*). He showed that, if the coefficients of the individual units' relations followed the RCR model, the aggregated coefficient estimate would not be biased. Theil (*6*) and Swamy (*4*) have considered the conditions under which the error from imperfect aggregation vanishes. The convergence of the error to zero is more likely and more rapid within the RCR framework than in the conventional fixed-coefficients model. Swamy, Barth, and Tinsley (*5*) criticized the validity of the rational expectations model because of the divergence between subjective and objective notions of probability. They suggested an RCR framework for aggregation

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[1] Italicized numbers in parentheses refer to items in the References at the end of this article.

over the potentially heterogeneous subjective probabilities of actors in the economy.

The problem of aggregation bias is here associated with differences in land quality and crop yield across counties. Although county-level analysis is feasible, the level of detail makes it difficult to reach general conclusions about regions or sectors. Combining county-level data into larger aggregates is a prerequisite to examining the distributional impacts of technological advance that may be conditioned by variation in land quality.

Alternative Aggregation Schemes

The analysis compares two aggregation schemes at the sub-State level, grouping counties by CRD's and by MLRA's. CRD's, as defined by the National Agricultural Statistics Service of the U.S. Department of Agriculture (USDA), are drawn within State boundaries in an apparently arbitrary fashion (splitting most Corn Belt States into a grid of eight or nine districts). MLRA's, as defined by USDA's Soil Conservation Service, do not necessarily follow State boundaries and are meant to coincide with similarities in land quality, taking into account variation in soil types and topography as well as weather patterns. (Note that, although MLRA's are defined without regard for county boundaries, the use of county yield data made it necessary to allocate split counties to one MLRA or another.) Thus, the initial hypothesis was that aggregation by MLRA's would be appropriate, but that comparison with CRD's would be useful because the CRD is a common unit of aggregation in economic analyses of crop production.

Illinois county data on corn yields were chosen for the empirical investigation of potential aggregation bias. Although the selection does impose some limits on the conclusions, it seemed a good place to start. Illinois has nine CRD's and all or part of five MLRA's. (I ignore small portions of two other MLRA's in the north (involving only six counties). The lack of congruence in Illinois between CRD and MLRA boundaries provides a good setting for tests of alternative aggregation schemes.

Model Specification

Similarity in the growth rate of corn yield is the criterion for grouping counties of comparable land quality. The model of yield behavior from 1950–80 is a simple one in which technological change, as represented by a linear time trend, is the sole determinant of growth. Alternative functional forms, such as the quadratic, were not suitable at the county level, where, at least in Illinois, corn yield has not yet reached a plateau. Such a plateau may appear in more aggregated data, and so a nonlinear time path might be appropriate. At the regional or national level, the appearance of a plateau may be attributable to the addition of more marginal lands to the production base rather than to a slowdown in technological advance.

The central hypothesis is that, under the correct aggregation scheme, the coefficient on the technology proxy variable (as well as the intercept) for each county within a grouping may be regarded as a drawing from a random distribution with an overall mean that is specific to the particular grouping. Where there is an overall CRD or MLRA growth rate and where a county's growth rate is equal to the group rate plus or minus some random deviation, the random coefficient model is appropriate. In the problem here, counties within a grouping are generally geographically contiguous, facing the same economic conditions (little variation in input or output prices) and the same agronomic and longer term climatic conditions. Thus, counties within a grouping should exhibit the same overall growth rate in yields over time (because of comparable rates of success from technology adoption). Individual deviations from this overall rate would be due to the influence of shorter term, random weather events from one year to the next. In contrast, a fixed-coefficient model of yield behavior would imply constancy over time in the differences among county trends, suggesting that permanent structural factors account for the variation.

I use two steps to test for aggregation bias under each classification scheme. First, for each grouping, I use a test to evaluate the hypothesis that county yield growth coefficients are fixed and identical, because, if they are, no aggregation bias would be present. If the null hypothesis of fixed and identical coefficients is rejected, the alternative hypothesis is that they are different, but it does not specify whether they are fixed and different or random and different. A second test determines whether the random coefficient model (random and different) is appropriate. If it is, no aggregation bias will exist (*1*, p. 545).

The specification of the econometric model for each grouping under the two aggregation schemes follows Swamy's formulation of the random coefficient regression framework (*3*). For an individual county i (where i = 1,...,N for each grouping and where N varies across groupings), the relationship between yield in each period (Y_{it}) and technology time trend (X_{it}) can be written as follows:

$$Y_{it} = X_{it}(\bar{\beta} + \mu_i) + e_{it} \qquad (t = 1,\ldots,T) \tag{1}$$

where:

$$E(e_{it}, e'_{it}) = \sigma_{ii} I \tag{1.1}$$

$$\beta_i = \bar{\beta} + \mu_i,\ E(\mu_i) = 0,\ E(\mu_i, \mu_i') = \Delta, \tag{1.2}$$

and $E(\mu_i, \mu_j') = 0\ (i \neq j)$

$$\beta_i \sim N(\bar{\beta}, \Delta) \tag{1.3}$$

Rewritten to include all NT observations for a particular grouping, the model is:

$$y = X\beta + Z\mu + e \tag{2}$$

where y has dimension NT × 1, X is NT × K, β is K × 1, Z is NT × NK, μ is NK × 1, and e is NT × 1. The covariance matrix for the composite disturbance:

$$\Phi = E\,[(Z\mu + e)\,(Z\mu + e)'] \tag{3}$$

is block diagonal, with block Φ_{ii} and X_i of dimension T × K,

$$\Phi_{ii} = X_i \Delta X_i' + \sigma_{ii} I \tag{4}$$

The generalized least squares (GLS) estimator for the model is a form of weighted least squares, where the weights are inversely proportional to the covariance matrices, as given by:

$$\hat{\bar{\beta}} = (X'\,\Phi^{-1}\,X)^{-1}\,X'\,\Phi^{-1}\,y \tag{5}$$

which can be written, as Judge and others show (*1*, p. 540), as:

$$\hat{\bar{\beta}} = \sum_{i=1}^{N} W_i b_i \tag{6}$$

where:

$$W_i = \left\{ \sum_{j=1}^{N} [\Delta + \sigma_{jj}\,(X_j'X_j)^{-1}]^{-1} \right\}^{-1} [\Delta + \sigma_{ii}\,(X_i'X_i)^{-1}]^{-1} \tag{6.1}$$

$$b_i = (X_i'X_i)^{-1} X_i' y_i \tag{6.2}$$

When Δ and σ_{ii} are unknown, they are replaced by the sample quantities:

$$\hat{\Delta} = S_b/N - 1,\ S_b = \sum_{i=1}^{N} b_i b_i' - 1/N \sum_{i=1}^{N} b_i \sum_{i=1}^{N} b_i' \tag{7}$$

$$s_{ii} = \tilde{e}_i \tilde{e}_i'/T - K \tag{8}$$

where the $\tilde{e}_i$ are the ordinary least squares (OLS) residuals. The sample quantity in equation 7 is used as an estimator of Δ in lieu of a larger expression involving the inverse of the X matrix (as given by *3*, p. 315), which may not be nonnegative definite. Judge and others (*1*, p. 542) suggest the use of the sample quantity (equation 7), which is always nonnegative definite and is unbiased, although possibly less efficient than Swamy's expression. The individual β_i can be recovered, following the discussion in Judge and others (*1*, p. 543), as an estimate of the group mean plus a prediction of the random component, μ_i, associated with a particular cross-sectional unit. This expression is:

$$\hat{\beta}_i = (\hat{\Delta}^{-1} + s_{ii}^{-1} X_i'X_i)^{-1} (s_{ii}^{-1} X_i'X_i b_i + \hat{\Delta}^{-1} \hat{\bar{\beta}}) \tag{9}$$

For any particular grouping of counties, the form of the composite disturbance, as given in equation 3, implies that each county may have a different error variance (which is constant through time) and that errors are uncorrelated across counties and through time. Such an assumption of independence is stringent, but is imposed because estimating the random coefficient model with more complicated error forms can require matrix inversion of order NT, which could be as large as 900 in the current application.

Tests for Aggregation Bias

I perform two tests to investigate the hypothesis of aggregation bias under alternative schemes for grouping Illinois counties by land quality based on similarity in the growth rate of corn yields. The first null hypothesis holds that the intercept and growth coefficients are identical within the group. If the null hypothesis cannot be rejected, the grouping will not suffer from aggregation bias. Performing the second test, which attempts to distinguish between models with fixed and different coefficients and those with coefficients that are random and different, is then unnecessary. When the random coefficient model is appropriate, no aggregation bias is presumed to exist. The forms of the two test statistics are given below, and tables 1 and 2 show results of their application.

The first test evaluates the possibility that all counties within a grouping exhibit the exact same rate of yield growth (and identical intercept), in which case the counties may reasonably be combined in further analysis of homogeneous land quality aggregates. The null and alternative hypotheses are given by:

Table 1—Testing for aggregation bias: Fixed and identical coefficients

Grouping	Calculated test statistic	Chi-square[1]	Number (N)
CRD1	47.21	40.28	12
CRD2	206.98	38.93	11
CRD3	130.25	32.00	9
CRD4	40.99	38.93	11
CRD5	26.99	26.22	7
CRD6	121.37	42.98	13
CRD7	338.51	48.28	15
CRD8	47.07	38.93	11
CRD9	101.39	38.93	11
MLRA108	124.61	95	30
MLRA110	128.98	37.57	11
MLRA113	55.12	32.00	9
MLRA114	251.65	40.29	12
MLRA115	963.90	>95	32

[1]Chi-square values at 1-percent level of significance with degrees of freedom equal to K(N – 1), where for each grouping K equals 2 and N is as given.

Table 2—Testing for aggregation bias: Random and different coefficients

Grouping	Calculated test statistic	Chi-square[1]
CRD1	119.84	
CRD2	183.63	
CRD3	136.32	
CRD4	101.46	
CRD5	78.63	11.34
CRD6	164.23	
CRD7	186.25	
CRD8	111.88	
CRD9	137.01	
MLRA108	7.53	
MLRA110	73.07	
MLRA113	34.68	11.34
MLRA114	126.79	
MLRA115	404.45	

[1]Chi-square value at 1-percent level of significance with degrees of freedom equal to 1/2 K(K + 1), where K equals 2 for all groupings.

$$H_o : \beta_1 = \beta_2 = \ldots = \beta_N = \beta$$
$$H_A : H_o \text{ is false} \tag{10}$$

The test statistic is distributed as chi-square with K(N – 1) degrees of freedom:

$$g = \sum_{i=1}^{N} [\, (b_i - \bar{\bar{\beta}})' \;\; X_i'X_i \, (b_i - \bar{\bar{\beta}}) \; / \; s_{ii}] \tag{11}$$

where:

$$\bar{\bar{\beta}} = (\sum_{i=1}^{N} s_{ii}^{-1} X_i'X_i)^{-1} \sum_{i=1}^{N} s_{ii}^{-1} X_i'X_i \, b_i \tag{11.1}$$

Table 1 shows the results of the application of the test to the nine CRD and five MLRA groupings as well as the value of chi-square at the 1-percent level of significance for K = 2 and the appropriate N for each grouping. The calculated value of the statistic g exceeds the chi-square value in each case.

These results lead to a rejection of the hypothesis of fixed and identical coefficients across counties within each CRD and MLRA grouping. However, a second test was required because the alternative hypothesis is not specific as to whether the coefficients are fixed and different or random and different. The validity of the random coefficient specification depends on the value of the elements of the variance/covariance matrix of the individual coefficient vectors, Δ (*4*, p. 122). The random coefficients model is appropriate if there is at least one nonzero element, but not if Δ is null. The null and alternative hypotheses are:

$$H_o : \Delta = 0$$
$$H_A : \Delta \neq 0 \tag{12}$$

To set up a critical region for this test, Swamy employs the asymptotic distribution of the likelihood ratio λ^*_c (*4*, p. 124) given by:

$$-2 \ln \lambda^*c = T \sum_{i=1}^{N} \ln\sigma_{ii} - (T - K) \sum_{i=1}^{N} \ln s_{ii}$$
$$- \sum_{i=1}^{N} \ln \mid X_i'X_i \mid \; - N \ln|N^{-1} S_b| \tag{13}$$

where:

$$\sigma_{ii} = [y_i - X_i\hat{\beta}(\sigma)]' \, [y_i - X_i\hat{\beta}(\sigma)] \; / \; T \tag{13.1}$$

and:

$$\hat{\beta}(\sigma) = [\sum_{j=1}^{N} \sigma_{jj}^{-1} X_j'X_j]^{-1} \, [\sum_{j=1}^{N} \sigma_{ii}^{-1} \, X_i'X_i b_i] \tag{13.2}$$

Under the null hypothesis, this statistic can be approximated as chi-square with 1/2 K(K + 1) degrees of freedom. Swamy suggests that one compute the estimates σ_{ii} with an iterative procedure, using the s_{ii} as starting values for the σ_{ii} in equation 13.2 and re-solving until the estimates of the coefficient vector and residual variances stabilize (*4*, p. 123). Here, for ease of computation,

the σ_{ii} in equation 13.1 are found directly from $\hat{\beta}(\sigma)$ where the σ_{ii} are replaced with their OLS estimates, the s_{ii}. The two approaches are equivalent asymptotically, although this approximation may be less efficient than the iterative method because it does not take into account the restriction that each b_i is an estimate of the same $\hat{\beta}(\sigma)$ (see *1*, p. 429).

In the application to the Illinois county data, the critical value of chi-square at the 1-percent level with three degrees of freedom is 11.34. This value is exceeded by all but one of the calculated test statistics for the nine CRD's and the five MLRA's. For MLRA 108, a value of 7.53 does not support the random coefficient model, and it suggests aggregation bias in that particular grouping. MLRA 108 (as shown in fig. 1, panel 2) cuts a large swath across the State, including some 30 counties. Based on an examination of soil maps and weather data, long-term rainfall patterns are different in the northern and southern parts of MLRA 108.

Aggregation Choice for Production Analyses

Results of the aggregation bias tests for corn yield data for Illinois counties support using CRD's in production analyses for which grouping by land quality is sensible. Supply data are often most readily available in CRD form, so the easiest solution may also be the best one. Designers of economic studies of production variation

Illinois Groupings

Crop Reporting Districts (CRD's)
Panel 1

Major Land Resource Areas (MLRA's)
Panel 2

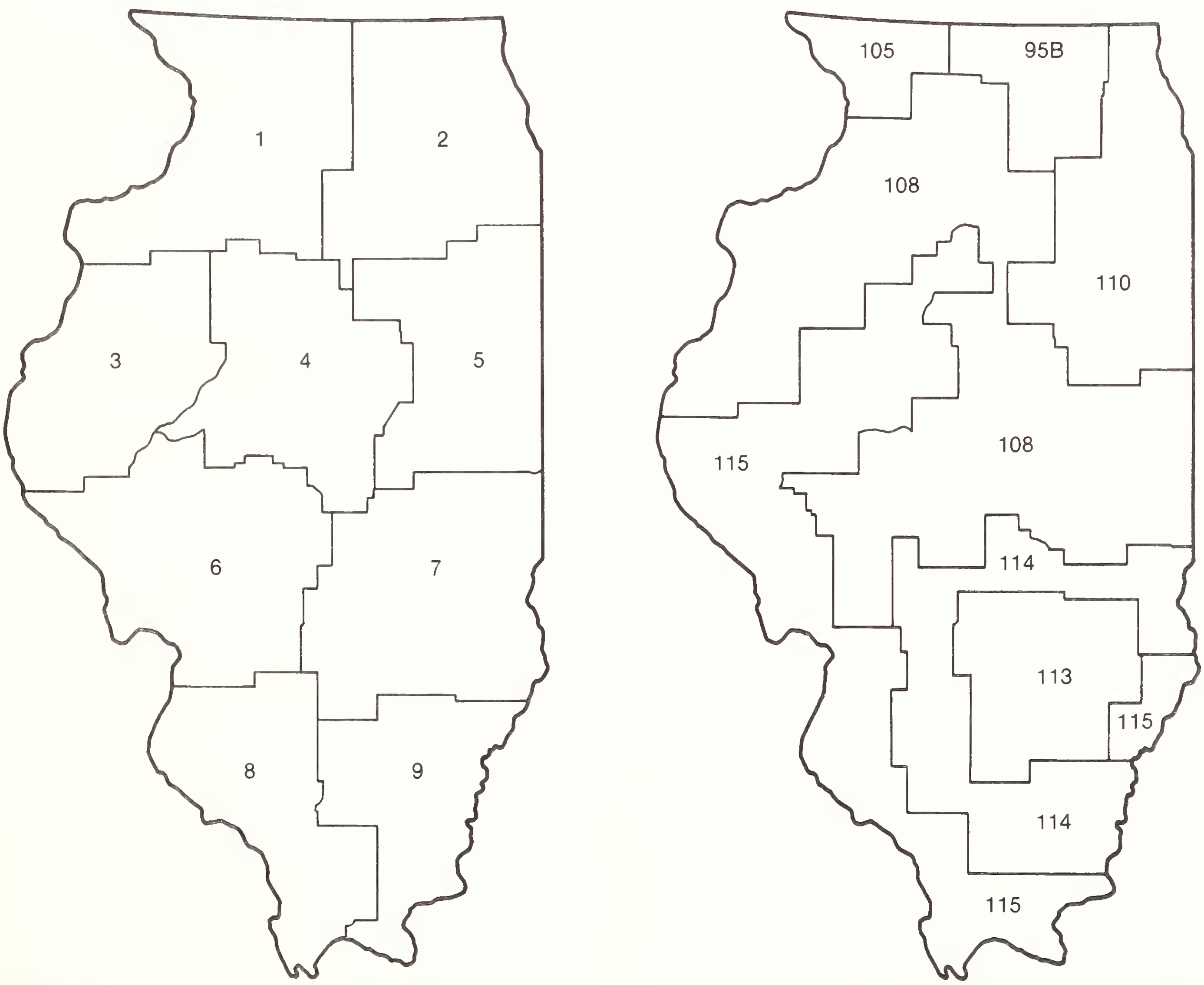

Table 3—RCR and EGLS estimates of overall yield growth rates

Grouping	RCR[1]	EGLS[2]
	Bushels per year	
CRD1	1.90	1.91
	{.10}	(.05)
CRD2	1.86	1.85
	{.09}	(.06)
CRD3	1.98	2.05
	{.14}	(.07)
CRD4	2.15	2.14
	{.12}	(.08)
CRD5	2.22	2.20
	{.13}	(.11)
CRD6	2.13	2.05
	{.02}	(.12)
CRD7	2.89	2.27
	{.12}	(.12)
CRD8	1.78	1.78
	{.09}	(.08)
CRD9	1.71	1.70
	{.09}	(.07)
MLRA108	2.20	2.16
	{.08}	(.04)
MLRA110	1.96	1.89
	{.10}	(.06)
MLRA113	1.91	1.92
	{.12}	(.09)
MLRA114	1.96	1.95
	{.13}	(.08)
MLRA115	1.87	1.85
	{.07}	(.04)

[1]Numbers in braces are measures of dispersion around overall mean.
[2]Numbers in parentheses are standard errors.

should consider the relevance of the MLRA definition, especially for State or sub-State analyses. When several States are to be considered, though, the consistency of the MLRA groupings across State boundaries might be appealing.

The test results do not provide definitive guidance on the choice of aggregation scheme. Because these tests reflect the experience of only one State, findings may be peculiar to Illinois, where variation in land quality does seem to coincide with CRD and some MLRA boundaries. However, according to the experience of some Illinois agricultural meteorologists who work more closely with county data than do most agricultural economists, this similarity within CRD's holds throughout the Midwest. This conclusion is based on qualitative perceptions, but it does raise the issue of statistical versus practical significance in testing for aggregation bias.

To demonstrate that the random coefficient model has made distinctions among yield growth rates that are of practical significance, one should examine the regression results. Table 3 reports the overall RCR mean growth rate coefficient for each CRD and MLRA grouping. The number in brackets beneath each coefficient is a measure of the dispersion of the individual county coefficients around the mean, not a conventional standard error. Each value is the square root of the appropriate element in the variance-covariance matrix of $\hat{\bar{\beta}}$, which is the first bracketed term in W_i in equation 6.1. Table 3 also shows the estimated generalized least squares (EGLS) estimate of a common, fixed coefficient for all counties in a grouping, allowing for different error variances across counties. The form of this estimator is given in equation 11.1. Table 4 shows OLS estimates of the yield growth coefficients and residual variances as well as the random coefficient estimates under both CRD and MLRA groupings for the CRD 3 counties (which also lie in either MLRA 108 or MLRA 115). CRD 3 illustrates the differences in coefficient estimates by estimator and grouping.

The RCR estimates of the overall mean growth rates are generally similar to the EGLS estimates of a common growth rate (table 3). In fact, the two are equivalent when Δ equals zero. The expression for $\hat{\bar{\beta}}$ as given in equation 5 can be written:

$$\hat{\bar{\beta}} = \left\{ \sum_{j=1}^{N} [\Delta + \sigma_{jj} (X_j'X_j)^{-1}]^{-1} \right\}^{-1} \left\{ \sum_{j=1}^{N} [\Delta + \sigma_{ii} (X_i'X_i)^{-1}]^{-1} b_i \right\} \quad (14)$$

and when Δ is equal to zero, the expression in equation 14 collapses to:

$$\bar{\bar{\beta}} = \left\{ \sum_{j=1}^{N} \sigma_{jj}^{-1} (X_j'X_j) \right\}^{-1} \sum_{j=1}^{N} \sigma_{ii}^{-1} (X_i'X_i) b_i \quad (15)$$

which is equivalent, when the σ_{ii} are replaced by the OLS estimates s_{ii}, to the EGLS estimator given in equation 11.1. But when Δ is not zero, the random coefficient model does not restrict all counties to the same growth rate. Moreover, it eliminates the possibility of aggregation bias.

Dividing the measure of dispersion of each coefficient (the numbers in braces in table 3) by the mean growth rate indicates the rate of dispersion of the individual county estimates from the overall mean. This type of coefficient of variation averages about 5 percent across all CRD and MLRA groupings. If more counties necessarily meant more heterogeneity (table 1), the level of variation might be correlated with the number of counties in each group. We observe no such variation in this sample. However, the range of values of the growth rate estimates does differ from CRD to MLRA grouping.

Table 4—OLS and RCR estimates of CRD 3 yield growth rates, by county

County	CRD 3 OLS[1]	CRD 3 RCR[2]	MLRA 108 RCR	MLRA 115 RCR	OLS s_{ii}
	Bushels per year				*Errors*
Henderson	2.37 (.16)	2.31	2.34	—	64.45
McDonough	2.12 (.25)	2.17	2.14	—	153.47
Knox	2.16 (.21)	2.19	2.17	—	111.92
Warren	2.45 (.18)	2.41	2.41	—	79.87
Hancock	1.95 (.28)	1.96	2.01	—	196.68
Adams	1.57 (.25)	1.67	—	1.76	153.51
Fulton	1.73 (.23)	1.82	—	1.85	135.41
Brown	1.82 (.19)	1.68	—	1.85	91.69
Schuyler	1.47 (.27)	1.65	—	1.73	180.65
Group RCR mean		1.98 {.14}	2.20 {.08}	1.87 {.07}	

— = Not applicable.
[1]Numbers in parentheses are standard errors.
[2]Numbers in braces are measures of individual county dispersion around the overall mean.

For CRD's, the lowest growth rate is 40 percent of the highest, whereas for MLRA's the lowest growth rate is only 15 percent of the highest. This contrast is likely attributable to the smaller number of counties in each CRD, which allows more sensitivity to individual deviations and prevents them from being "washed out" as in larger MLRA aggregates.

The results in table 4 show the effects of alternative estimators (and alternative assumptions about aggregation) on growth rate estimates for individual counties. The hypothesis of fixed and identical coefficients across counties was rejected for CRD 3 (table 1), reflecting the heterogeneity in the OLS estimates of the growth coefficients. The OLS residual error variances shown in the last column also vary, providing justification for allowing heteroscedasticity across counties in the RCR and EGLS estimates. The results of the second test supported the validity of the random coefficient model for CRD 3. Given that the OLS and individual RCR growth rates are similar, is estimating the random coefficient model necessary? Yes, because a conventional F test would lead to rejection of the hypothesis of fixed and identical coefficients and to the conclusion that aggregation by CRD was inappropriate. But the test for aggregation bias in the RCR framework demonstrates that the differences across coefficients represent random deviations for an overall mean and that these counties can indeed be combined in empirical analysis.

Conclusions

I used a random coefficient regression framework to evaluate an aggregation scheme for corn yield data at the county level. I used similarity in growth rates of corn yield (taken as a proxy for land quality) as the criterion for comparing a grouping of counties by CRD's with a grouping by MLRA's. Using historical Illinois data, I tested OLS, EGLS, and RCR models to describe corn yields. When the RCR model is valid for a grouping, no aggregation bias exists.

Empirical findings for Illinois supported the CRD scheme, whereas the MLRA approach raised questions about the composition of some groups. Closer examination of the regression results showed that estimates of growth rates of corn yield could be similar across the estimates. However, one can draw conclusions about the existence of aggregation bias only by explicitly considering the validity of the RCR model.

The issue of the most appropriate unit of aggregation to use when one analyzes crop production data has not been settled. The application of the empirical testing to Illinois data only limits the generality of the conclusion about the relative merit of the CRD versus the MLRA schemes. However, because agricultural production data are quite often available only in CRD form, analysts may be encouraged by the apparent acceptability of the CRD groupings. A definitive answer would require that alternative schemes be evaluated across and within States.

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P's and Q's from Palgrave

Palgrave's Dictionary of Political Economy. Inglish Palgrave's *Dictionary of Political Economy* appeared in volume form sequentially in 1894, 1896 and 1899. However, 1894 was not the year in which the *Dictionary* began publication. . . . It was not until 1908, when the appendix to the third volume was published, that its publication could be said to have been complete. It took seventeen years to effect the publication of the *Dictionary*—better than twenty years of work if one takes into account the fact that the contractual agreement between Palgrave and Macmillan is dated 1888.

M. Milgate
The New Palgrave, Vol. III, p. 791

(See review on p. 34.)

Effects of Income Distribution on Meat Demand

William F. Hahn

Abstract. *Shifts in the distribution of income tended to increase the demand for beef and decrease the demand for pork and chicken in the early eighties. However, shifts in relative prices and other factors worked to decrease the demand for beef. Consequently, the demand for beef declined from 1980 to 1985. Despite only limited information on income distribution, one can estimate its effect on the demands for beef, pork, and chicken. The article shows how income distribution will affect market demand given the functional form selected for consumer demand.*

Keywords. *Consumer demand, income aggregation, beef, pork, chicken.*

The distribution of income is presumed to affect the demand for beef, pork, and chicken. The effects of the distribution of income on demand can be measured with a limited number of variables. I have estimated and tested a set of demand functions based on this analysis.

My results suggest that a person's pork consumption changes little as income changes and that a person's chicken consumption tends to grow at approximately the same rate as income. Shifting income from one person to another will do little to affect the demand for pork and chicken. Beef consumption also grows with a person's income. However, beef consumption is more sensitive to income changes for people with lower incomes. Decreasing the unemployment rate and transferring money from the rich to the poor will increase demand for beef.

Literature Review

Economic theory holds that consumer demand is a function of a consumer's tastes, income, and the prices of the goods purchased. The market demand for a consumer good is the sum of consumers' demands. Economists often estimate market demands using average consumer income as an explanatory variable. However, in only a few cases is average quantity demanded a function of average income only. In most cases, the market demand for a good depends on both the distribution of income and its average level. Market demand functions that are a function of only the average consumer income can be misspecified.

Theoretical discussions of the problem of aggregating individual demands into market demands appear in a number of sources. Two notable examples are Gorman (*3*) and Muellbauer (*4*).[1] Both discuss aggregation of demand systems consistent with the theory of utility maximization. Gorman demonstrated that, if all consumers' demands for all goods are linear in income, the average purchase of any good can be written as a function of average income, regardless of the distribution of income. If the demand for a good is not linear in income, that good's average demand will not generally be a function of average income.

Muellbauer demonstrated cases where the average amounts of all goods consumed in a market can be written as the demands of one consumer with some representative income. The representative income is the mean income only when consumers' demands are linear in income.

Articles by Berndt, Darrough, and Diewert (*1*), Blinder (*2*), Van Doorn (*6*), and Simmons (*5*) are examples of applied work where the problems of income distribution are addressed.

Berndt, Darrough, and Diewert were primarily interested in comparing three demand systems with one another. Each met Muellbauer's criteria for the existence of a representative income. Berdnt, Darrough, and Diewert used information on proportions of consumers in income classes to approximate and eliminate the effects of aggregation bias.

Berndt, Darrough, and Diewert included effects of income distribution simply for the sake of theoretical consistency. Simmons was interested in actually measuring the effects of the income distribution on demand. Simmons had data on the proportion of British national income going to each of five income classes. He also had

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[1] Italicized numbers in parentheses refer to items in the References at the end of this article.

data on aggregate British consumption and prices. He assumed that each member of a class had the same income and the same demand function. He worked out the implications for market demand. His study suggested that each income class had different demand functions, but that the demands of each class were linear in expenditure.

Blinder found that the distribution of income had an important effect on aggregate consumption. However, the effect was not the one he expected. His estimates showed that the rich had a higher propensity to consume than the poor. He concluded that this result was reasonable given the nature of his data. Blinder wished to measure the distribution of income. He actually measured the distribution of earnings of people who worked. He did not count the earnings of those without jobs. As women and youths began to participate in greater numbers of low-paying jobs, Blinder's measure of inequality increased. However, because the people entering low-paying jobs had been earning nothing, income inequality may have declined.

Van Doorn also attempted to measure the effects of the distribution of income on consumption. His study is interesting because it used the same type of demand function as I used here. He made strong assumptions about the distribution of income and its effect on consumption. His data suggested that information on income distribution did little if anything to improve the consumption estimate. The result is not surprising considering that he misspecified the effects of income distribution on demand. He erroneously showed that average consumption was related to the geometric mean of income. I show that the relationship between consumption and the income distribution is a more complicated function of the income distribution.

In the four applied studies mentioned above, the researchers used information on the class distributions of income or earnings to measure the effects of income aggregation on market demand. In this study, I used a limited number of variables to estimate the effects of income dispersion on demand.

Aggregation of Linear in Logarithm Demand Functions

Assume that consumer i's demand for good Q can be written:

$$\ln(Q_i) = B0 + B1\ln(P_1) + \dots + Bj\ln(P_j) \dots \quad Bn\ln(P_n) + By\ln(Y_i) \tag{1}$$

where the subscript i refers to the i'th consumer; P_j is the price of the j'th good; Y_i is the i'th consumer's income; and Bj and By are the consumer's elasticities of demand with respect to the j'th price and income, respectively. Assume also that all consumers have identical demand functions and face identical prices. Assume also that consumers have different incomes. Note that equation 1 implies that the quantity demanded of good Q can be expressed:

$$Q_i = \exp(B0 + B1\ln(P_1) + \dots Bn\ln(P_n) + By\ln(Y_i)) \tag{2}$$

where the function exp( ) is the base of the natural logarithms raised to the power in the parentheses.

Equations 1 and 2 are demand equations for individual consumers. Many of the data on consumption are time series data showing total consumption or per capita consumption for large groups of consumers. The consumption information is aggregated over consumers. Consider the average quantity demanded by all consumers in the market. The average quantity purchased is:

$$\bar{Q} = \exp(B_0 + B1\ln(P_1) + \dots + Bn\ln(P_n)) \sum_{i=1}^{M} \exp(By\ln(Y_i))/M \tag{3}$$

where R is the number of consumers and $\bar{Q}$ is the average consumption of good Q. The average consumption of good Q is a function of the prices of goods 1 through N and of every consumer's income.

For equation 3 to be estimated with aggregate time series data, one would need a series of observations on each consumer's income in the market. In most empirical studies using linear in logs demand functions, the logarithm of the average quantity purchased is written as:

$$\ln(\bar{Q}) = B0 + B1\ln(P_1) + \dots + Bn\ln(P_n) + By\ln(\bar{Y}) \tag{4}$$

For equation 4 to follow from equation 3, the following equality must hold:

$$\sum_{i=1}^{R} \exp(By\ln(Yi))/M = \exp(By\ln(\bar{Y})) \tag{5}$$

Equality 5 is true only if one of the following three conditions holds: (1) if the income elasticity of demand for good Q is exactly 1, (2) if the income elasticity is exactly zero, or (3) if all consumers have the same income. If none of these conditions is met, equation 5 is invalid. Replacing the left side of equation 5 with the right side, when consumer demands are estimated, can

cause aggregation bias, and the estimates of the price and income elasticities of demand may be biased.

The problem facing the researcher is how to eliminate the potential aggregation bias with only limited information about the distribution of income. Under certain circumstances, limited information may be enough. The distribution of income is a statistical distribution. Many statistical distributions are specified with one or two parameters. If the functional form of the income distribution is fixed over time, simply knowing the values of some relevant parameters could be enough to deal with the income aggregation problem.

The average quantity demanded, equation 3, can be specified as a function of the moment-generating function of income. Moment-generating functions are often used with statistical distributions as a relatively simple way to derive a distribution's mean, variance, and other moments. The distribution of income can be treated as any other statistical distribution. The moment-generating function, M(w), is an explicit function of the variable w and an implicit function of a statistical distribution. The moment-generating function for the variable X is defined as:

$$M(w) = \sum_{i=1}^{A} \exp(wX_i)/A \tag{6}$$

In equation 6, A is an indexing number representing the total number of the X's. Now consider the left side of equation 5. It is the moment-generating function for the log of income evaluated at By:

$$\sum_{i=1}^{R} \exp(By\ln(Y_i))/M = M(By) \tag{7}$$

Over time, the income distribution may shift, which will also shift the moment-generating function. Assume there is a set of variables, represented by the vector Z, that contains information about the distribution of income. The variables in Z may be the parameters of the distribution of income or variables that could be used to derive those parameters. The moment-generating function is a function of the income distribution information vector Z as well as the variable w, so that the moment-generating function of the log of income may be written M(w,Z).

$$\ln(Q) = B0 + B1\ln(P_1) + \ldots + Bn\ln(P_n) + \ln(M(By,Z)) \tag{8}$$

The demonstration above shows that when consumers' demands are log linear, market demand can be written as a function of the moment-generating function of income. Given the moment-generating function and the appropriate data, estimating market demand functions based on equation 8 would be fairly straight-forward. However, I had no *a priori* information on the appropriate form for the moment-generating function. Therefore, I specified a simple, *ad hoc* form for the moment-generating function. The form I selected is consistent with statistical theory.

The moment-generating function for the log of income must meet the following two restrictions:

(1) $M(1,Z) = \bar{Y}$ for when $w = 1$, $\exp(1^*\ln(Y_i)) = Y_i$

(2) $M(0,Z) = 1$ for when $w = 0$, $\exp(0^*\ln(Y_i)) = 1$

The first restriction implies that, when the income elasticity is 1, the log of the moment-generating function will be the log of the average income. When the income elasticity is 1, one needs to know only the average income to correctly specify the market demand. The second restriction implies that when the income elasticity is zero, the log of the moment-generating function is also zero and the market demand is independent of the level or distribution of income.

The information about the income distribution used in this study is the mean disposable income deflated by the CPI, $\bar{Y}$, the ratio between the mean and median family income, R, and the unemployment rate, U.

$$M(w,Z) = \bar{Y}^w \exp[\,(w^2 - w)\,(\,A\ln(R) + B\ln(U) + C\ln(R)^2 + D\ln(U)^2 + E\ln(R)\ln(U) + F)\,] \tag{9}$$

The interpretation of equation 9 is fairly straightforward. The variables R and U determine the shape of the income distribution, whereas $\bar{Y}$ determines the absolute level of the income distribution.

Yearly observations have been collected for the 1960–84 period. The per capita disposable income, the CPI, and the yearly unemployment rates are U.S. Department of Commerce data, and the mean and median family incomes are reported by the Census Bureau. Per capita disposable income is calculated as a residual from the national income accounts whereas the mean and median family incomes are derived from household surveys. Per capita income has trended upward though the sample period. The unemployment rate has generally increased since 1960, peaking in the early eighties and declining slightly toward the end. Throughout the sample period, mean family income has exceeded median income. This relationship implies that the income distribution is skewed toward higher incomes, with a few very large family incomes causing the mean to be larger than the median. The ratio was fairly stable throughout the early

part of the sample period. However, since the late seventies, the two measures of family income have diverged. Real median family income has actually declined, whereas real mean income has grown.

Substituting equation 9 into equation 8 and introducing a random component, e, gives:

$$\begin{aligned}\ln(Q) = {} & B0 + B1\ln(P_1) + \ldots + Bn\ln(P_n) \\ & + By\ln(\bar{Y}) + (By^2 - By)\,(A\ln(R) + B\ln(U) \\ & + C\ln(R)^2 + D\ln(U)^2 + E\ln(R)\ln(U) + F) \\ & + e \qquad (10)\end{aligned}$$

Equation 10 is a function of prices, average income, and the variables R and U. Except for the parameters B0 and F, the parameters of the model are exactly identified. One can estimate the model using least squares regression. However, if demand equations for more than one commodity are estimated, the parameters of the model are over-identified and one can estimate the equations simultaneously. The simultaneous estimation should increase the efficiency of the estimates.

Equation 10 defines the model developed in this article. Equation 10 will be referred to as the Moment-Generating Function model or the MGF model.

Demand functions based on the MGF model were estimated for beef, pork, and chicken. The dependent variables are per capita disappearances of beef, pork, and broiler meat. These variables were regressed against deflated average retail prices of beef, pork, and broilers and the moment-generating function. The quantity and price data were taken from USDA statistics.

The independent variables also included a trend and trend-squared terms. The time trend variable starts at −1.2 in 1960 and advances one-tenth each time period. The value of the trend variable is zero in 1972, the midpoint of the observation period. I included the trend terms to pick up the influences of excluded variables, such as changes in tastes or shifts in the income distribution that are unexplained by the income ratio and the unemployment rate.

As noted, the moment-generating function I specified is *ad hoc*. To evaluate the effectiveness of the MGF model we have compared it with two alternative hypotheses. The first alternative, alternative 1, excluded the ratio and unemployment terms from the regression analysis. If shifts in the distribution of income are important and if the specified moment-generating function is able to account for those shifts, then the MGF model should perform significantly better than alternative 1.

The MGF model also implies a series of cross-equation restrictions on the effects of the variables R and U. Alternative 2 tested the significance of the cross-equation restrictions on the variables R and U. If the MGF model is correct, alternative 2 will not perform significantly better than the MGF model.

Results

I estimated the MGF model and its two alternatives using a full information maximum likelihood package from the PC-based econometrics program, SORITEC. The estimation algorithm did not converge after more than 400 iterations. I "renormalized" the data by replacing the log income ratio and the log unemployment rate with their deviations from their mean values. The renormalization allowed the algorithm to converge in fewer than 50 iterations.

Table 1 contains the estimates of the coefficients of the renormalized MGF model and the results of the hypothesis tests. Table 2 shows the estimates for the alternative models. I compared the models with one another using the log likelihood ratio test. The log likelihood ratio test has an asymptotic chi-square distribution. The test statistic comparing the MGF model with alternative 1 is much larger than the 5-percent critical value, while the test statistic comparing the MGF model with alternative 2 is much smaller than the 5-percent critical value. Alternative 1 is rejected compared with the MGF model, whereas the MGF model is accepted compared with alternative 2. I conclude that the MGF model is a success. The distribution of income has a statistically significant effect on meat demand. I also conclude that the MGF model measures the effects of the income distribution.

Comparing alternative 1 with the model also allows one to investigate the effects of aggregation bias on the estimated elasticities of demand. The price and cross-price elasticities are similar for both sets of equations. However, the beef and chicken income elasticities are much different. Aggregation bias seems to have decreased the estimated income elasticities for beef and chicken.

The hypothesis tests imply that the distribution of income has a significant effect on demand. The variables R, U, and their cross-products are significant as a group. However, none of the estimates of the parameters of the moment-generating function is significant at the 5-percent level.

Each demand equation contains at least one significant trend variable. The trend implies that there are strong forces affecting the demand for meats that have not been captured by my analysis.

Table 1—Demand parameter and elasticity estimates and hypothesis test

Item	Estimate	Standard error	
Beef equation:			
Constant	1.294*	0.609	
Beef price	−.580**	.054	
Pork price	.076	.039	
Broiler price	.049	.038	
Income	.924**	.172	
Trend	−.114**	.040	
Trend squared	−.108**	.018	
Pork equation:			
Constant	3.972**	.132	
Beef price	.437**	.059	
Pork price	−.784**	.046	
Broiler price	.071	.046	
Income	−.016	.036	
Trend	.013	.015	
Trend squared	−.060	.012	
Broiler equation:			
Constant	3.637**	.427	
Beef price	.150	.082	
Pork price	−.075	.061	
Broiler price	−.140*	.062	
Income	−.057	.119	
Trend	.312**	.031	
Trend squared	−.020	.018	
Moment-generating function parameters:			
A	−83.993	191.639	
B	−1.519	3.323	
C	−192.052	426.467	
D	.566	1.066	
E	621.031	1419.160	
Log likelihood	201.08		
	Test statistic	df	5-percent critical value
Comparisons:			
MGF vs. alternative 1	36.62	5	11.070
MGF vs. alternative 2	9.88	10	18.307

* The coefficient estimate is significant at 5 percent.
** The coefficient estimate is significant at 1 percent.

Table 2—Parameter estimates for alternative models

Item	Alternative 1		Alternative 2	
	Estimate	Standard error	Estimate	Standard error
Beef equation:				
Constant	3.489	−0.941	1.885	0.902
Log beef price	−.436	−.075	−.586	.057
Log pork price	.138	−.064	.096	.454
Log broiler price	.103	−.066	.055	.039
Log income	.308	−.266	.757	.255
Trend	.045	−.058	−.073	.059
Trend squared	−.116	−.027	−.120	.024
ln(R)	C		4.713	4.391
ln(U)	C		.095	.026
ln(R)ln(R)	C		11.510	9.265
ln(U)ln(U)	C		−.063	.052
ln(R)ln(U)	C		−35.443	31.815
Pork equation:				
Constant	3.729	−0.705	5.045	1.136
Log beef price	.405	−.056	.421	.072
Log pork price	−.796	−.048	−.760	.057
Log broiler price	.064	−.049	.064	.049
Log income	.052	−.199	−.325	.322
Trend	−.007	−.043	.077	.074
Trend squared	−.065	−.020	−.094	.031
ln(R)	C		−.346	5.528
ln(U)	C		−.051	.033
ln(R)ln(R)	C		3.622	11.665
ln(U)ln(U)	C		−.004	.066
ln(R)ln(U)	C		5.297	40.057
Chicken equation:				
Constant	1.562	−1.104	3.154	1.525
Log beef price	.025	−.088	.163	.097
Log pork price	−.130	−.075	−.068	.077
Log broiler price	−.190	−.077	−.127	.066
Log income	.526	−.312	.085	.432
Trend	.165	−.068	.302	.099
Trend squared	−.009	−.032	.007	.041
ln(R)	C		−9.117	7.425
ln(U)	C		−.075	.044
ln(R)ln(R)	C		−26.146	15.667
ln(U)ln(U)	C		.021	.089
ln(R)ln(U)	C		59.382	53.797
Log likelihood:	182.77		206.02	

C in a coefficient's place means that it was constrained to zero.
Blanks indicate not applicable.

Conclusions

The problems of aggregation of individuals into markets has received some attention in the economic literature. However, the problem is seldom addressed in applied studies. The problem of aggregation is often ignored because of data deficiencies. The technique used here allows one to estimate the effects of income aggregation using incomplete information. Using the unemployment rate and the ratio of mean to median family incomes provides a simple, but usable proxy for measuring the effects of the distribution of income on demand. The sensitivity of this analysis could be improved with more complete time series information on income distribution.

This approach to correcting aggregation bias could easily be applied to other demand systems and to the study of the demand for other commodities. The technique could be generalized and adapted to the study of supply as well as demand. Accounting for the effects of aggregation provides an important link between the economic theory of individual behavior and its application to aggregate data.

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More P's and Q's

prices and quantities. These are the most directly and readily observable attributes of commodities (goods and services produced for and exchanged on the market). Both price and quantity relate to a unit (piece, bushel, barrel, pound, etc.), established usually by commercial practice as the customary unit of reckoning.

The intrinsically numerical character of prices and quantities renders accounts and statistics, the incessant measurement of the stream of commodities, feasible. This preoccupation is motivated by and yields motivation to business and economic interests. It also seems to be responsible for the profound drive to develop economic theories with the aid of mathematical tools, applied already successfully to the exigencies of natural sciences.

S. Brody
The New Palgrave, Vol. III, p. 957

(See review on p. 34.)

Book Reviews

Price Shocks and Energy Models

Macroeconomic Impacts of Energy Shocks. *By B.G. Hickman, H.G. Huntington, and J.L. Sweeney (eds). Leiden and New York: North-Holland Publishing Company, 1987, 331 pp., $73.25.*

Reviewed by John M. Reilly

The book provides a reasonable introduction to the major macroeconomic models and analyzes similarities and differences among model responses to a set of macroeconomic shocks as well as potential policy responses to these shocks. It represents the working group report of model comparisons conducted by the Energy Modeling Forum (EMF) over an 18-month period in 1982 and 1983. The macroeconomic working group (EMF-7) follows a tradition of energy model comparisons dating to the results of EMF-1, published in 1977. EMF-7 includes the well-known, large U.S. macroeconomic models with limited detail on energy (Wharton, Chase, DRI, Bureau of Economic Analysis, Michigan Annual Econometric, and MIT-PENN-SSRC). These models do not disaggregate by energy fuel. Therefore, substitution among fuels is not explicitly addressed. The number of models was expanded by the inclusion of several that have slightly different focuses: International macroeconomic linkages (LINK and the FRB Multi-Country Model), world oil markets and U.S. economic growth (Mork Energy-Macroeconomic and Hubbard-Fry), small monetarist models (Claremont and St. Louis FRB), long-term macroeconomic growth (Hickman-Coen), and a Canadian model (MACE) that contrasts energy impacts in a small open economy.

The value to model users of controlled experiments across models is indisputable, but the process is difficult, time-consuming, and uninteresting to all but the narrowest of audiences. With impetus from the Energy Information Administration of the U.S. Department of Energy, requiring strict standards for model validation, support from the Electric Power Research Institute, and the careful, continuing efforts of the Energy Modeling Forum, the energy modeling community has taken the lead in validating and comparing economic models. In this sense the workshop report provides a contribution to economic analysis and is justifiably a part of the North-Holland Series with the title *Contributions to Economic Analysis*. The book's lasting contribution is its documentation of the results of EMF's attempt to compare models and as an introduction to that process. Economic modelers in areas other than energy would do the profession a favor by imitating the EMF process.

The book is essential reading for the business and policy community that uses the large macroeconomic forecasting services. To their credit, the editors do much more than edit, providing a careful framework for comparing results and model structures without serving as proponents for or antagonists of particular approaches. The obvious audiences are those involved in energy policymaking and modeling. For them the book provides insights into how the economy responds to an oil or natural gas price shock and how monetary and fiscal policy may be used to limit inflationary or employment impacts.

The group pursued two broad goals: "First, we sought to understand the models themselves by identifying important commonalities as well as structural differences. Second, we sought to use the models to sharpen our understanding of energy shocks and of the related policy issues" (p. vi). It is difficult to compare model structures and simultaneously provide convincing policy guidance. The editors make it clear that they are aware of the difficulty. The reader, however, never gets a satisfactory answer to the question: Are we comparing models or understanding the economy? The reader is, therefore, unsure if the results indicate weaknesses of the models or provide fundamental insights into the behavior of the economy.

A simplified, reduced-form econometric model attempting to directly estimate responses to past oil shocks might have provided a benchmark comparison with the summary elasticities derived from the model experiments. Such an exercise would have provided a direct comparison with actual economy performance. The difficulty the reader faces is illustrated by the discussion of

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four questions for which energy economists have sought answers for at least the past 15 years:

First, are the impacts of a drop in oil prices simply the opposite of a price increase? According to the editors, "the model results suggest that when the economy is experiencing significant unemployment, the economic gains from a modest oil price reduction of $7 per barrel or less are equal but opposite to the losses induced by a price increase of comparable size" (p. 4). The workshop participants did not consider other base employment conditions or other price drop magnitudes; they hesitated to extend their results to the more severe 1986 price drop. I suspect model results under other conditions would suggest roughly equal, but opposite, impacts because the underlying production and energy demand relations do not capture structural features that could produce unequal effects.[1] Thus, the finding is more indicative of the model structures than it is a finding about how the economy actually operates.

Second, does an oil price shock permanently affect economic growth? According to the editors, "the more slowly adjusting models would presumably eventually recover to long-run equilibrium were the simulation horizon extended beyond four years" (p. 173). Although growth effects are somewhat afield of the EMF-7 primary objective of shortrun impacts, more could have been made of any differences in predictions between the longrun growth models and the shortrun macroeconomic models. Three simple characterizations of the impacts of energy shocks on growth are possible. One possible impact is that the oil price shock results only in the unemployment of resources with no change in investment. Thus, potential Gross National Product (GNP) grows undisturbed. Once full employment is restored, the economy is back on track. A second type of impact would include a temporary effect on investment. With time to adjust, investment would regain its preshock level as would economic growth. However, the economy would not make up for the investment it lost during the shock/recovery phase, making potential GNP less than if the shock had not occurred. A third possibility is that the oil price shock would affect relative prices over time as well as across commodities, thereby affecting longrun savings and investment decisions and altering the growth rate of the economy. If potential GNP is unaffected as in the first case, a focus on short-term macroeconomic effects provides an accurate picture of the full impact of the energy shock. If either potential GNP or the growth rate of the economy is affected, short-term impacts may be swamped by growth effects.

Third, how has the experience of the past 15 years changed the economywide response to an oil shock? Most of the models were estimated with 1960–80 data, with the data for a few models extending back further. There is considerable evidence that the private sector increased its ability to respond to an oil shock (for example, penetration of dual-fuel capabilities). Attention to structural elements that might explain time-varying price responses could shed some light on whether these macro models tend to overestimate shock impacts, given the experience gained as a result of two major oil shocks.

Fourth, how certain are we about the responses of the economy to an oil shock? According to the editors, "there was wide-spread agreement that a 50-percent oil price shock would severely reduce U.S. real output and international purchasing power" (p. 111), but there were substantial differences in the estimated magnitude of the response of the economy of Federal policy to alleviate the negative effects of the shock. As macroeconomists, the model developers had strong and diverse views on macroeconomic policy. These differing views translate into substantial predicted differences in the economy's response to the Government's fiscal and monetary policy. The model developers were not energy economists, and they generally adopted similar and relatively simple representations of energy use and production. The participants disagreed in the area in which they were relatively expert and agreed in the area in which they were relatively inexpert.

These issues are not faults of the book, which provides a background against which the issues can be raised. If the book is flawed in this area, it is because the editors did not raise the issues as directly as they might have and because they were overly cautious in bringing the simulation results to bear on the issues.

A particularly useful result of the study is the clarity with which the editors remind us that GNP is a limited measure of economic impacts in an open economy, particularly with a foreign oil shock, because GNP fails to account for the terms of trade effects. These effects represent a real loss of the Nation's purchasing power abroad not measured in unemployment and lower GNP. The editors also show that a foreign oil shock would affect even an energy-independent United States. The macroeconomic models clearly demonstrate that these energy shocks are transmitted indirectly to the United States through international trade. Such effects are likely to be missed in models limited to the energy sector that deal only with energy trade.

The book's 125 pages of overview are meant to be accessible to a broad audience, whereas the remainder of the book offers greater detail. The overview is long and, although the terms used are relatively well de-

[1] See, for example, Paul C. Stern (ed.), *Improving Energy Demand Analysis* (Washington, DC: National Academy Press), 1984.

fined, it will be tough going for the reader lacking graduate training in economics or familiarity with large-scale macroeconomic models. The chapters offering greater detail do not offer that much more detail, conveying a sense of redundancy.

The major disappointment of the book is that, although the discussion of the models highlighted the value of having a diversity of types of models (long-term growth, quarterly macroeconomic, energy detail, and the Canadian model), the comparison of the results never took advantage of that diversity. To what extent do some models have comparative advantages for certain uses because of the original focus of the modelers? Did the participants feel that the long-term models gave a better picture of recovery from the shock? Are there features of the long-term models that might improve the quarterly models? Should we put more faith in the models with an energy focus because we are dealing with an energy issue? Where do the rational expectations models fit, particularly in terms of the money supply policies explored?

More P's and Q's

plutology (Gr. πλοῦτοσ, wealth). This term was used by Courcelle-Seneuil to describe that part of his treatise on political economy which dealt with what is described by some more modern writers as 'pure theory'; that scientific study of the results of the action of economic motives on men and societies to which the terms 'economics' and 'economic science' have been applied in the effort to escape the confusions which arose from embracing under the general title 'political economy', both these more abstract investigations and the application of the knowledge thus gained, with that derived from other sources, to problems of practical statesmanship. To this second part of the subject the eminent French economist applied the term *Ergonomy.* The Australian W.E. Hearn adopted the title for his work, *Plutology, or the Theory of the Efforts to Satisfy Human Wants.*

Reprinted from *Palgrave's Dictionary of Political Economy*

A.W. Flux
The New Palgrave, Vol. III, p. 897

(See review on p. 34.)

Small Banks: A Matter of Survival?

The Future of Small Banks in a Deregulated Environment. *Donald R. Fraser and James W. Kolari. Cambridge, MA: Ballinger Publishing Company, 1985, 262 pp., $32.*

Reviewed by Stephen W. Hiemstra

The rash of bank failures in the eighties occurred shortly after the passage of the Depository Institutions Deregulation and Monetary Control Act of 1980 and the Garn-St. Germain Depository Institutions Act of 1982. These acts worked to eliminate interest rate ceilings on time deposits and other restrictions on banks and thrifts. Did deregulation increase the failure rate of small banks in the early eighties, and can small banks survive in a more competitive market for financial services unencumbered by regulations on interest rates, product offerings, and location? Fraser and Kolari in *The Future of Small Banks in a Deregulated Environment* conclude that small banks are unlikely to be disadvantaged in the new market environment.

The authors review the role of small banks in the U.S. financial system, the origins of banking regulation, and the performance of small banks over the past two decades. They analyze the consequences of interest rate, product, and geographic deregulation; financial innovation and technological change; trends in bank riskiness; and strategies for coping with a more competitive market with a focus on the implications of deregulation for small banks.

Fraser and Kolari's focus on small banks is timely and topical. Small banks provide 40 percent of all loans to small businesses and are critical in the agricultural areas of the Midwest and South. Small banks have, therefore, suffered from the same financial stress as agriculture in the eighties. The link to small businesses means that small banks play an important role in creating employment in selected areas of the country. According to the authors, 85 percent of all banks have deposits of less than $100 million.

Fraser and Kolari report that deregulation is widely believed to affect small banks more than large banks because of their heavy reliance on deposits as a source of loanable funds, perceived economies of scale, and a lack of diversified portfolios. They show that small banks have been successful in securing other sources of loanable funds in the eighties, that fairly small banks can obtain economies of scale in banking, and that portfolio management has not contributed heavily to small bank losses. Small banks may adjust slowly to rapid changes in the market environment, but they also have the advantage of being better capitalized than large banks. Furthermore, small banks (less than $50 million in assets) consistently earned a higher rate of return on assets than large banks did in every year from 1970 to 1982. The authors, therefore, do not view small banks as having significant liabilities in operating in a more competitive market environment. The key for success in the eighties, as they see it, lies in the ability of small banks to develop strategies for meeting consumer demand, adjusting to new technology, and controlling costs.

Although the conclusion that small banks are not likely to be disadvantaged by a more competitive market is plausible, why—if small banks are better capitalized than large banks—did so many of them fail in the early eighties? The authors observe the difference in capitalization and use it to argue that small banks have the potential to grow more rapidly than large banks in the coming decade, but they never analyze why this difference arises. In view of the importance of the difference in capitalization in the conclusions they reach, this oversight is significant.

Several years have passed since the book was published. The literature on the deregulation of financial markets continues to expand, but Fraser and Kolari's book stands almost alone in dealing with deregulation's effects on small banks. The presentation is thorough and scholarly. Researchers and students will appreciate the book's review of empirical data and the literature on the economics of banking, even if its conclusions can only be tentatively accepted.

The reviewer is a financial analyst with the Agriculture and Rural Economy Division, ERS.

Linking Agricultural and Environmental Policy

Agriculture and the Environment. *By Tim T. Phipps, Pierre R. Crosson, and Kent A. Price (eds.). Washington, DC: Resources for the Future, 1986, 298 pp., $10.*

Reviewed by Marc O. Ribaudo

Agriculture has profoundly affected the environment since people first began to farm. Agriculture converts large areas of land to crops, increases the potential for soil erosion, and introduces chemicals into the environment. As clean air, clean water, undisturbed ecosystems, and wildlife become scarcer, the conflicts between agriculture and the environment take on greater importance. Governments are often caught in the middle of such conflicts by trying to satisfy many interests. Environmental policies have been adopted to protect environmental resources. But agricultural policies and programs have been adopted that encourage agricultural production and promote the use and degradation of environmental resources. Conflicts in environmental and agricultural policies need to be addressed in books such as this collection of conference papers.

The National Center for Food and Agricultural Policy sponsored a conference on Agriculture and the Environment in April 1986 that was designed to address the state of knowledge about policy-relevant links between agriculture and the environment and to identify research needs and priorities. The book was derived largely from the major papers and formal discussions presented at the conference. The editors had four goals: (1) to provide an overview of environmental problems associated with agriculture, (2) to establish a framework for the evaluation of problems and policies, (3) to consider policy alternatives, and (4) to discuss and evaluate policies.

The chapters by the editors present an overview of the issues and summarize the policy-relevant lessons. The other chapters are separate papers on environmental problems confronting agriculture and issues in policy analysis. Most chapters are followed by a formal discussion by another expert.

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The papers include: (1) "Agriculture and the Environment: An Overview" by Tim T. Phipps and Pierre R. Crosson, (2) "Soil Erosion and Policy Issues" by Pierre R. Crosson, (3) "Irrigated Agriculture and Mineralized Water" by Robert A. Young and Gerald L. Horner, (4) "Problems of Pesticide Regulation: Health and Environment Versus Food and Fiber" by Erik Lichtenberg and David Zilberman, (5) "Pesticides and Public Policy: A Program for Research and Policy Analysis" by John M. Antle and Susan M. Capalbo, (6) "Incentives for Agricultural Development of U.S. Wetlands: A Case Study of the Bottomland Hardwoods of the Lower Mississippi River Valley" by Randall A. Kramer and Leonard A. Shabman, (7) "Institutional and Neoclassical Approaches to Environmental Policy" by Alan Randall, and (8) "Induced Innovation in Agriculture and Environmental Quality" by C. Ford Runge.

Several authors deal with specific environmental problems associated with agriculture: soil erosion, pesticide use, wetlands conversion, and dissolved minerals. In a book of this type, I would have expected each chapter to have the same purposes as those for the book. The authors succeed to varying degrees. Crosson, in the chapter on soil erosion, does a good job of outlining the problems associated with soil erosion, and he makes a strong case for placing more emphasis on offsite impacts than on onsite impacts. Crosson also contrasts voluntary vs. regulatory programs for inducing farmers to reduce soil erosion, and he discusses the merits of dealing with erosion at the site of offsite damages, rather than on the field where erosion occurs. A discussion of how current agricultural policies and programs (such as price supports, cross-compliance, and the other provisions of the Conservation Title of the 1985 Food Security Act) might affect the amounts of erosion generated by agriculture would have strengthened the presentation. Crosson should have discussed the Conservation Reserve Program, which is being touted in some corners as the most environmentally beneficial farm program in history.

In their discussion of irrigated agriculture and mineralized water, Young and Horner present a complete picture of the salinity problems in the West and the surrounding policy issues. They present a lengthy outline of the nature of the problems, the role of water policy in creating the current problems, and the conflict between optimal policy and current water law.

Pesticide use and environmental issues are discussed in two chapters. Lichtenberg and Zilberman explore several issues on the tradeoffs between agricultural productivity and environmental quality and human health. They present a framework for estimating the productivity of pesticides, taking into account pest resistance. Resistance to pesticides is an important component in the derived demand for pesticides. Lichtenberg and Zilberman demonstrate how the failure to account for the social cost of pesticide use leads to overuse. They discuss some current policies and practices that encourage the overuse of pesticides, such as the emphasis on product appearance. Their presentation could have been strengthened considerably by a section on the impacts of pesticides on human health and the environment. They do present a framework for estimating the costs to human health from pesticide use, but suggest no such framework for estimating environmental costs. Economists have at their disposal tools for estimating the value of various nonmarket goods, including environmental quality. One should not forget it was concern over the environmental impacts of pesticides that led to Carson's *Silent Spring* and to an increased awareness of environmental issues.

Lichtenberg and Zilberman present Integrated Pest Management (IPM) as a technology that would "induce farmers voluntarily to narrow—if not close—the present gap between the farmers' interest and the social interest in pest management . . ." (p. 138). However, IPM does not bring us any closer to solving the problem of getting farmers to account for social costs in their production decisions. IPM alters only the demand for pesticides, so overapplication from a societal standpoint would still be a problem. Recommendations for policy actions addressing the apparent conflicts between current agricultural policies and environmental policies would have been a logical conclusion.

The editors must also have concluded that the issue of pesticides and environment required more coverage, because they added a paper by Antle and Capalbo that was not presented at the conference. Antle and Capalbo address the effect of pesticides on health and the environment, and they present a framework for studying pesticide regulation. It measures the benefits and costs of pesticide use to agriculture and the benefits and costs of pesticide use to society, and it suggests interventions when social benefits do not equal or exceed social costs.

Kramer and Shabman's paper on the agricultural development of wetlands is the most technical. However, the economist will find it an excellent piece of research. Kramer and Shabman use a simulation model to assess the economic feasibility of converting bottomland hardwood forests in the lower Mississippi Valley into cropland, given policies and laws such as the agricultural and forestry tax codes, agricultural price and income support programs, and wildlife habitat incentives. They show how the profit motive affects private resource decisions, and they demonstrate that policymakers need a good understanding of economic relationships to design effective policies.

Randall's paper on institutional and neoclassical approaches to environmental policy was interesting, but it did not match the stated conference objectives. Randall did not discuss specific policies, but presented reasons why different schools of economic thought arrive at different policy recommendations. He concluded that policymakers are left to make decisions in the face of conflicting advice and that it is not yet clear how the decision process might be strengthened. It would have been useful to see evidence that conflicting advice from economists is why many policy decisions are made without consideration for economic efficiency.

Runge's paper on adaptive innovation presents a framework for analyzing technological change, environmental quality, and forces that direct the formation of environmental policy. Runge concludes that environmental goods become more valuable relative to agricultural products as a country develops and national income rises. His discussion might have been extended to include a more explicit description of the link between institutional change and environmental quality. Runge could have also devoted less space to explaining why environmental issues are currently of major concern. That environmental quality is a major concern in this country is a fact, and the basic premise of the book.

I found the discussants' reviews of the individual papers quite useful. The discussants generally included examples and expanded on the authors' arguments.

The final chapter by the editors summarizes the conference policy findings and identifies the common themes: the roles of efficiency and equity in formulating policy, the need to fill gaps in knowledge about the links between what happens on the field and in the environment, the need to place values on environmental commodities, and the need to evaluate the environmental effects of current price support and subsidy programs. However, the editors could have strengthened their summary by evaluating policy alternatives and by making their own recommendations. They do not discuss

important subjects such as groundwater contamination, biotechnology, and low-input agriculture. The book itself would be more complete if a chapter dealing with these subjects were added.

The editors did not include one of the most useful portions of the conference in the book. The conference concluded with an excellent panel discussion that added several interesting perspectives to the proceedings: a discussion of the importance of the Conservation Reserve Program, the potential role of low-input agriculture, the potentials of biotechnology, the effect of global economic forces on domestic farm decisions, and various policy options for protecting the environment. A summary of the panel discussion would have made the proceedings more useful.

The book could help both economists and noneconomists who want to review some of the environmental and policy issues facing agriculture. However, they will need to search elsewhere for a detailed evaluation of specific problems and policies.

More P's and Q's

quotas and tariffs. Until the mid-1960s, it was commonly argued that quotas and tariffs were equivalent protective devices in terms of their effects on the volume of imports, domestic price, domestic output, and domestic consumption. Yet, at times they have been viewed as non-equivalent, as for instance revealed by the relative lenience of GATT rules toward tariffs vis-à-vis quotas.

Bhagwati (1965) initiated the discussion on the comparative properties of tariffs and quotas and showed that the equivalence result is restricted to cases that are characterized by competitive market structures. In the context of a partial equilibrium model, he demonstrated that the presence of monopoly power in production and/or in quota holdings would lead to a breakdown of the equivalence proposition. Since then, the relationship between quotas and tariffs has been examined within the general equilibrium framework and the non-equivalence result has been demonstrated under a variety of conditions, such as uncertainty and retaliation.

N. Cagatay
The New Palgrave, Vol. IV, p. 32

(See review on p. 34.)

Marketing: Public and Private Style

Agricultural Marketing Enterprises for the Developing World. *By John C. Abbott (ed.), Cambridge, England: Cambridge University Press, Aug. 1987, 217 pp., $44.50.*

Reviewed by Leo V. Mayer

The importance of marketing in the food chain has long been an issue in agricultural economics. Not, however, in the mind of John Abbott. In his view, marketing is an active, not a passive, activity that can stimulate additional agricultural output, raise farm income, provide services consumers want, earn foreign exchange through exports, and contribute to overall development. The 26 case studies in this fascinating book show how marketing accomplishes all these activities. The studies range from the experiences of the Botswana Meat Commission to the Zimbabwe Cotton Marketing Board.

Although Abbott focuses on developing countries, economists in developed countries will find the book useful, too, especially in the United States, where the marketing of farm products has, until recently, been the forgotten "limiting factor" in agricultural profitability. Abbott shows that we have much to learn about marketing techniques, and much can be done to improve farm profits by dissecting and applying marketing principles. Such analysis is especially important for countries with a significant reliance on exports.

Abbott believes that governments often discourage producers from attending to the marketing end of the food chain. Governments intervene in the market chain to stabilize prices, but the next step is frequently to nationalize marketing enterprises or to set up state enterprises to carry out marketing activities. The result is nearly always the same, whether in the United States or elsewhere: a government takes over the marketing function to the detriment of farmers and consumers. U.S. readers have only to recall the paucity of domestic cheese varieties until recently to see the stultifying effects of government intervention on private initiative. In turn consumers demand imports to get variety, imports replace domestic supplies, and surpluses appear as markets are protected from the lower prices that would result from foreign competition.

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Many of the various authors' recommendations for expanding the role of marketing in developing countries also apply to advanced economies. For example, Abbott credits E.W. Cundiff with a set of useful steps that improve the marketing process:

1. Change the negative attitudes of producers toward marketing,
2. Develop marketing curricula in educational institutions,
3. Promote specialized practical training for marketing managers,
4. Invest in marketing infrastructure, and
5. Maintain freedom of entry together with measures to restrict collusion.

The widespread applicability of these steps is obvious, but achieving them is no easy matter.

Other case studies show the initiative of individuals when allowed to carry out marketing activities without government interference. The role of government marketing boards in improving the market process is also described, although private enterprise would appear to demonstrate more success. The advantages cited for private enterprise include personal initiative, ability to make quick decisions, independence of spirit, and persistence.

The book offers a wealth of examples to readers interested in reviewing alternative marketing systems. It illustrates how local marketing conditions can be decisive in determining the most appropriate enterprise. This same message has arisen out of the recent burst of interest in small farmer marketing enterprises in the northeastern United States. Success stories in Massachusetts and neighboring States illustrate the opportunities available when marketing specialists look for new

initiatives and opportunities. Leadership for these State activities seems to have come more from State governments than from academic sources.

The most important contribution the book can make to improving farm profitability may be its emphasis on practical experience rather than theory. Alerting marketing specialists to various marketing opportunities and techniques may lead them to rethink the appropriateness of current marketing curricula. Assigning first- or second-year students to read the 26 case studies would introduce them to the variety of ways to market farm and food products. In the process, we could perhaps begin to shift more of the responsibility for marketing from governments to individuals. Both government and individual farmers would be better off.

End of the P's and Q's

Quesnay. The concept of net product characterizes Quesnay's entire economic analysis and not just the *Tableau.* For instance in the 1957 article *Grains* he analyses the distribution of surplus between social classes; the Physiocratic theory of taxation is also built on the distinction between surplus and means of production. The notion of net product was the main analytical tool used by Quesnay to examine all the other important economic features in society. The idea of a single tax on rent is closely related to the determination of surplus. Rent is landlords' revenue, the landlords receiving the highest share of the net product of cultivation.

Agriculture is the only productive sector, and taxation of surplus is the only way of avoiding damage to future production. The stock of productive capital must not be reduced by taxes, otherwise it would be impossible to maintain the same level of activity as the previous year. Hence rent is the only taxable magnitude.

G. Vaggi
The New Palgrave, Vol. IV, p. 27

(See review on p. 34.)

The New Palgrave: A Dictionary of Economics, Volumes 1–4. *John Eatwell, Murray Milgate, and Peter Newman (eds.) New York: Stockton Press, 1987, 4,103 pp., $650.*

Reviewed by Gene Wunderlich

Think of a basic term or expression in economics. Chances are you will find an informative essay on the subject in *The New Palgrave* (NP). I commend this extraordinary publication to readers of the Journal while recognizing that not many persons are likely to read a 4,000-page word book from Abbott to Zoning. Entries by more than 900 authors include most of the familiar topics such as demand, supply, and prices (several of each, in fact). It is the less familiar topics, however, such as Keynes' parable of the widow's cruse, that may be more novel and useful to agricultural economists. All entries deal with their subjects in considerable detail. Coverage of the economics vocabulary is extensive, perhaps complete. I was unable to find "J curve," but it may be in there somewhere. More than a third of the entries are biographies of past and living economists. Word lists, indexes, and cross-references assist the searcher.

Agricultural economists may enjoy the excellent genealogy of their discipline prepared by Karl Fox. He relates the development of agricultural economics to the development of agriculture in the United States. The extended essay examines the contributions of many of the founders of the discipline in universities and the U.S. Department of Agriculture, such as Henry C. Taylor, Edwin Nourse, John Black, and Oris V. Wells. He intentionally focuses on a narrow core of agricultural economics including production and demand, supply, and price analysis, leaving derivative subjects such as resource economics and community development to other essayists. His remarkable essay is extensively referenced (especially to Fox) and provides an overview of the profession that all new ag economists should read.

The editors of the NP announce their slant toward "theoretical and applied aspects rather than descriptive and institutional detail." They support their announcement by confining the scholarly, but expansive, Warren Samuels to an essay of less than two pages on institutional economics. Thomas Sowell's biographical entry on Veblen is less than distinguished. Essays on more recent subjects such as rational expectations, risk, or moral hazard appear more welcome and are extensively presented. Each essay contains an opening definition, a body of the main theoretical arguments, a background and setting, and a list of references. Most entries are written at a level requiring more than a lay comprehension of economics.

"Dictionary," in the case of the NP, is something of a misnomer. Entries have neither diacritical marks nor boldface emphases, so saying "tatonnement" aloud requires the help of a French dictionary. The 2,000 or so entries in the NP comprise an encyclopedia, not a dictionary in the usual use of the term. Viewed as an encyclopedia, the NP is clearly the standard for books of this type.

There are over 280 dictionaries and encyclopedias of economics. These references serve a wide variety of needs. Two recent dictionaries of economics, one compiled by Pearce and another by Moffat, for example, contain definitions without the extensive explanation, interpretation, and references of the NP and consequently are more compact. By contrast, the NP has spared no space to produce *the* definitive vocabulary of economics and has succeeded admirably.

Both the original and the new Palgraves profess "to provide the student with such assistance as . . . to understand the position of economic thought at the present time." At 22.5 pounds and $650 retail, the NP is neither portable enough nor affordable enough for most students. Furthermore, encyclopedias and dictionaries are reading tools, and they are not intended to provide an overall structure to a whole discipline needed by students. As a reading tool, however, the formidable NP should be available to students on the shelves of all libraries and reading rooms. It should be in the bookcases of researchers and dedicated readers of economic literature. Readers of the Journal will want to make sure that the NP is available to them.

The reviewer is an agricultural economist with the Resources and Technology Division, ERS.

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